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JOULE AND THE STUDY OF ENERGY

BY

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CLASSICS OF SCIENTIFIC METHOD

GENERAL INTRODUCTION

"Sachez également Messieurs . . . que la science dans notre siècle, est l'âme de la prospérité des nations et la source vive de tout progrès ! Sans doute, la politique avec ses fatigantes et quotidiennes discussions semble être notre guide . . . vaine apparence ! Ce qui nous mène ce sont quelques découvertes scientifiques et leurs applications."—PASTEUR.

THE manifold activities of the human mind in its attempt to comprehend the universe have left us a glorious heritage. The student, be he youth or adult, who wishes to trace the growth of understanding in Music, in Literature, or in the graphic arts, finds no lack of guidance. Excellent reproductions of the great masterpieces of painting or of sculpture can be obtained at small cost in volumes where the improvement in technique and the development of new ideas are indicated by a friendly hand. Scientific invention has made the study of great masterpieces of music possible to all, and there is now growing up an adequate library of musical appreciation ; while the great works of literature probably suffer from over-interpretation.

The masterpieces of Science are not, however, so accessible to those who must of necessity dwell in the outer courts of her temple. The Alembic Club reprints, Ostwald's *Klassiker*, and Messrs. Gauthier-Villars's *Les Maîtres de la Pensée Scientifique* provide the specialist in various branches of science with convenient copies of epoch-making scientific papers, but they do not help the layman. Many educated men and women of to-day feel a desire to know more about the scientific interpretation of the external world. The specialist in one branch of science is little more than a layman in others, and he too

desires a wider acquaintance with the working of the scientific spirit throughout the ages. The enlightened teacher of science realises that he has failed in his duty if he has done no more than communicate certain scientific facts to his pupils. There is, in short, a demand for a new literature of scientific appreciation.

The aim of this series is to provide reproductions of the great masterpieces of science in convenient form, together with a complete account of the action and reaction of ideas which, through the process of time, led up to the crucial experiments carried out and described by some great master. Biographical details will be introduced, and an attempt will be made to show the various social and other influences as they assist or retard the growth of knowledge. It is hoped that a reader who takes up a volume of the series, dealing with a branch of science of which he is ignorant, will be able, without further aid, to trace the steps by which the human mind has passed from chaotic ignorance to ordered knowledge.

E. R. T.

NEWCASTLE-ON-TYNE.

PREFACE

MOST prefaces would more aptly be termed postscripts, and this one is no exception. The aim of this little book is so obvious that explanation is unnecessary, and the sources from which I have drawn my material are indicated in the Bibliography. My task has been made easier by the editor, who has contributed many helpful suggestions and criticisms; by my wife, who read the proofs and prepared the Index; and by my daughter, Eleanor, who assisted in the preparation of the diagrams.

ALEX. WOOD.

EMMANUEL COLLEGE,
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PART I.—INTRODUCTION TO THE PROBLEMS STUDIED BY JOULE

CHAPTER I

INTRODUCTORY

RESISTANCE

A VERY little consideration will show us that throughout the activities of any day of our life we are largely concerned with overcoming all sorts of resistances. Everything we lift has to be lifted against a resistance which we call its " weight." Everything we pull or push has to be moved against a force which resists its motion. If we cycle we have to move the machine and ourselves against a resistance—a resistance of which we become acutely conscious if there is an adverse wind. Taking a wider view, we find the same thing to be true of the activities of the world as a whole. All the various processes of industry are carried out by elaborate machinery, and all this machinery works continually against a resistance. It requires considerable force to overcome the resistance—a force which may be supplied by steam-power or electric-power—and the machinery ceases to function the moment this force is removed. There is a resistance to motion in the case of our railway trains and ocean liners, and in order that passengers and goods

may be transported this resistance must be overcome. The stone of which our houses are built was cut into shape against a resistance, was transported by road or rail against a resistance, was lifted into its place in the building against a resistance. Obviously, then, it is convenient to have some word to express this almost universal process of overcoming resistance, and so we give it the name " work."

Work

The popular use of this term is wide and loose. We speak of manual work and brain work, and use it for all forms of physical and mental activity. Its scientific use is restricted, however, to the overcoming of a resistance, and this lends itself readily to a method of measuring work. We measure it by multiplying together the magnitude of the resistance and the distance through which it is overcome. The easiest resistance to use for purposes of measurement is the resistance which is felt when a body is lifted—*i.e.* the weight of the body. We therefore agree to take as our unit the work which must be done to lift a mass weighing one pound through a vertical height of one foot, and agree to call this one foot-pound of work. This is the term used by engineers. In using this unit we assume that the work done in raising one pound two feet is the same as that done in raising two pounds one foot—two foot-pounds in each case. This assumption may not appear to be borne out by our sensations. We may be conscious of more expenditure of effort in lifting a heavy weight than a light one, even when the distances are adjusted so as to make the work equal according to our definition. Our sensations, however, are always unsatisfactory standards of measurement, and all our experiments with mechanical devices

bear out the assumption. If, then, we know the resistance (or the force necessary to overcome it, which is always equal and opposite to the resistance) and the distance through which it is overcome, we can always calculate the work done. For instance, by fixing the hook of a spring-balance to the handle-bars of my bicycle and attaching a rope to the head of the balance I find that to pull the bicycle along at a speed of about ten miles per hour while some one sits on the saddle, requires the exertion of a force equal to the weight of ten pounds. Thus the resistance to the motion of my bicycle at this speed is a force of about ten pounds weight. Every mile that I travel then involves the performance of $10 \times 5280 = 52,800$ foot-pounds of work. To reach the first floor of my house from the ground floor I have to mount seventeen steps, each 7 inches high—or almost exactly 10 feet. As my weight is 150 pounds the amount of work I perform every time I go upstairs is 1500 foot-pounds. It is important to note here that the distance involved must always be measured in the direction in which the force acts. All weights act vertically downwards, and therefore it is always the vertical height through which a weight is raised which comes into our measurement. As far as the lifting of my weight is concerned the work done will be the same, whether I reach the first-floor of my house from the ground level by climbing up a vertical ladder inside the house or by walking up a long sloping plank reaching from the ground to a first-floor window, or by proceeding upstairs in the usual way, although the length of my path will be different in the three cases.

Energy

And now we must pass on to consider another quantity closely related to work and now known by the name of

"energy," although the word was first used in this sense by Lord Kelvin centuries after the thing itself was known. Energy is the capacity for doing work, so that whatever is capable of overcoming a resistance is said to possess energy. Let us consider one or two simple cases. In order to keep the mechanism of an old eight-day clock going, a resistance must be overcome and work must be done. In order to secure this, we "wind up" the clock, raising to the top of the case the one or more clock-weights which it contains. In this position the clock-weights possess energy, since in slowly falling to the bottom of the case as the clock "runs down" they overcome the resistance of the wheelwork and keep it, and the pendulum, in motion. Energy which depends on the position of the body is called "potential" energy, as is also the energy which depends on the distortion of a body. For example, our watches are kept going by the overcoming of a resistance, and therefore by the expenditure of energy. When we wind them up at night we coil the mainspring tightly. In this position the spring possesses energy which it loses as it gradually uncoils, the energy being used up in this case also in work done on the wheelwork of the watch.

Now let us consider a different kind of energy. Suppose it has become necessary for us to nail up a box. The resistance to the nails is very great; we cannot press them in. If we take a hammer—even a very heavy hammer—and lay the head on top of the nail, nothing happens, although the hammer head possesses some potential energy. If, however, holding the hammer in the approved way, we bring the head down smartly on the nail, it is driven in through an appreciable distance, and an appreciable amount of work is done on it. The energy available for this depended, not on the position of the hammer head when it struck the nail, but on its motion.

It was what we call " kinetic " energy, or what the old natural philosophers called *vis viva*. These two forms of energy, kinetic and potential, we find in many familiar phenomena. There are still to be found in many places mills which are worked by water. In these the work of the mill is performed by the energy of the water in a stream. They will be found to be of two types (*a*) the overshot wheel, where the water is led on to the top of the wheel and is caught in little troughs attached to the wheel; these empty as they come to the bottom and, rising empty, are continually overweighted by the full troughs on the descending side: (*b*) the undershot wheel, used in rapidly moving water where the stream is discharged against fixed paddles attached to the wheel, which is carried round in consequence. It is not difficult to see that in the first case the energy used is mainly potential, and in the second case mainly kinetic.

POWER

We are now reaching the end of our preliminary survey of mechanical quantities, but we must not pass from this part of our subject without some consideration of what is meant by the term " power " when used in the sense of " horse-power." What exactly do we mean when we speak of a "four horse-power" motor cycle? A schoolboy on being asked the question replied without hesitation that the cycle had an engine which could drive it as fast as four horses could do. When it was suggested to him that four horses could hardly be expected to draw the cycle much faster than two, he changed his ground and said that the engine could drive the cycle four times as fast as a single horse could drag it. Now the boy was quite right in thinking that there is a connexion between horse-power and speed. High-power cars are all potential

"road-hogs." But he was quite wrong in thinking that the connexion is a simple one. The horse-power of an engine measures the rate at which it can do work. From experiments made with actual horses it was found that for a prolonged spell they could do work at the rate of 550 foot-pounds per second, or 33,000 foot-pounds per minute. Hay is frequently carried from the farm cart to the top of the stack into which it is being built by a moving belt carrying long teeth. The hay is placed on this by one man standing on the cart while two or three others on top of the stack distribute the hay over the top as it arrives. The mechanism is frequently worked by a horse which is harnessed to a pole and walks steadily round and round in a circle. If the horse comes up to the standard of those which were used to fix the unit (as I believe very few do) and if the machinery works without friction (which it is very far indeed from doing), then $60 \times 33{,}000 = 1{,}980{,}000$ foot-pounds of work could be done in lifting the hay per hour; so 198,000 pounds could be lifted through a height of 10 feet in this time. Much of the work of the horse, however, is used merely in overcoming the friction of the machinery, and a correspondingly less amount of useful work can be done.

In the case of my cycle I perform, as previously indicated, 52,800 foot-pounds of work per mile when going at a speed of 10 miles per hour. I therefore do 52,800 foot-pounds in 6 minutes, or 8800 foot-pounds per minute. But 1 H.P. is equivalent to 33,000 foot-pounds per minute, so that I am developing about ¼ H.P. This I can of course maintain for a considerable period. On the other hand, I can run upstairs in less than 2 seconds, performing the necessary 1500 foot-pounds of work. In doing so I am working at the rate of 45,000 foot-pounds per minute—*i.e.* developing about 1½ H.P. Needless to say, I could not maintain this effort for very

long. Returning to the bicycle, it will be seen that *if* I were to fit to it an engine giving ½ H.P., and *if* the engine did not increase the resistance to motion, and *if* increased speed made no change in this resistance, then I should be able to travel at 20 miles per hour instead of 10 miles per hour. It must be remembered, however, (1) that a more powerful engine means a heavier engine, and more resistance to motion, and (2) that resistance to motion is not independent of speed but gets much greater as the speed increases. From these two considerations it follows that so far from the speed being directly proportional to power a small increase of speed may require a very considerable increase of power.

CHAPTER II

THE SEARCH FOR PERPETUAL MOTION

MACHINES

BEFORE we can understand what the men who sought for Perpetual Motion were really bent on finding, we must consider what a machine is and what it does. In its simplest form it is a contrivance to assist man in the performance of work. A lever is designed to enable a man to overcome a resistance which he could not overcome directly. By means of a crowbar a man can move an obstacle upon which he could unaided make no direct impression. By using a system of pulleys a man can raise into position on the wall of a house a stone which several men together could not lift. Armed with a pair of nut-crackers a child can crack a walnut—a feat which only the strong-fingered would attempt by direct pressure. From these comparatively simple machines we can pass by easy stages to the extremely complicated machinery which we find in any modern factory—machinery wonderfully adapted to the tasks it has to perform. In every case, however, we find that before the machine can do its work it must be linked up to some source of energy. It must be worked by a man, a steam engine, an electric motor, or some other source of power. Nor is it sufficient that it should be merely started in motion. It must be kept in motion. Why should it not be possible to devise a mechanism which would develop its own power and

which, if once started, would continue indefinitely to perform its allotted task ?

PERPETUAL MOTION

In this way there suggested itself to the human mind one of the two great objects of search which have lured men on and made them sacrifice time, money, and even life itself. The first of these was the philosopher's stone —a process whereby the baser metals might be transmuted into gold. The idea underlying the second object of search is much less familiar. The perpetual motion machine was to be, not merely a machine which would maintain itself in motion, but a machine which would maintain itself in motion *against a resistance—i.e.* which would maintain itself in motion and at the same time perform work. Now the latter of these two would be incomparably the more useful discovery. The man who discovered a process of converting lead into gold would no doubt become rich, but he would become rich at the expense of other people and would confer no benefit on the human race. Gold has a value for us merely because we have agreed to regard it as a claim upon the service of others. The possession of gold enables me to demand the services of my baker, my grocer, my tailor, the railway company, etc., and so to satisfy some of my needs and wishes. Yet no one desires gold for its own sake. If we found ourselves on a desert island with millions of sovereigns they would have lost all their value. We could neither eat them nor procure food with them, and wearing them as ornaments would very soon pall on us. On the other hand, the discovery of perpetual motion would potentially enrich the whole human race. It would mean the discovery of an inexhaustible supply of energy. It would lead to the performance of all the

work of the world on much easier terms than is possible to-day. Hence the eagerness with which the search has been pursued and the ingenuity which has been expended in the design of the various machines proposed. In no single case have the labours been crowned with success. It is not always easy to explain exactly why the machine does not work, but the fact remains that after the initial impulse has spent itself, the machine stops until another impulse starts it again. An interesting account of the various attempts will be found in *The Seven Follies of Science*, by John Phin. Here we shall only notice one or two of the most famous.

The Hydrostatic Paradox

Ideally simple is the arrangement shown in Fig. 1, and thus described in Dr. Arnott's *Elements of Physics*. "A projector thought that the vessel of his contrivance represented here was to solve the renowned problem of perpetual motion. It was goblet-shaped, lessening gradually towards the bottom until it became a tube bent upwards at C and pointing with an open extremity into the goblet again. He reasoned thus: A pint of water in the goblet A must more than counterbalance an ounce which the tube B

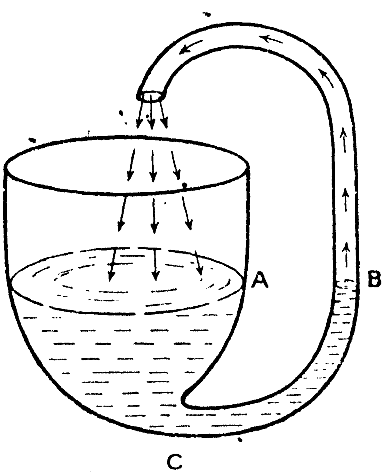

Fig. 1.—Perpetual motion device.

will contain, and must therefore be constantly pushing the ounce forward into the vessel again at A and keeping up a stream or circulation which will cease only when the water dries up. He was confounded when a trial showed him the same level in A and B." Now had this device worked at all, it would have done all that we could wish. For the water in falling back again into the goblet could have been made to turn a small wheel and do work. Unfortunately the arrangement violates a fundamental law of hydrostatics—that liquids always tend to find their level, and, it may be added, to keep their level when they have found it.

Marquis of Worcester's Wheel

One of the historic efforts is that of the Marquis of Worcester, and his machine is typical of a very large number. The idea and the success which attended the effort is thus described by the Marquis himself in his *Century of the Names and Scantlings of Inventions*, published in 1663, four years before his death. Article 56 (see Plate I.) reads as follows: "To provide and make that all the Weights of the descending side of a Wheel shall be perpetually further from the Centre, then those of the mounting side, and yet equal in number and heft to the one side as the other. A most incredible thing, if not seen, but tried before the late King (of blessed memory) in the Tower, by my directions, two Extraordinary Embassadors accompanying His Majesty, and the Duke of Richmond and Duke Hamilton, with most of the Court, attending Him. The Wheel was 14 foot over, and 40 Weights of 50 pounds apiece. Sir William Balfore, then Lieutenant of the Tower, can justifie it, with several others. They

all saw, that no sooner these great weights passed the Diameter-line of the lower side, but they hung a foot further from the Centre, nor no sooner passed the Diameter of the upper side, but they hung a foot nearer. Be pleased to judge the consequence."

No sketch of the wheel is appended, and no further reference is made to it in the *Century of Inventions*, but it is dealt with in Dirck's *Life, Times, and Scientific Labours of the Second Marquis of Worcester*. He gives a suggested design for the wheel, with the following description: "Let the annexed diagram represent a wheel of 14 feet in diameter, having 40 spokes, 7 feet each, and with an inner rim coinciding with the periphery, at 1 foot distance all round. Next provide 40 balls or weights, hanging in the centre of cords or chains 2 feet long. Now, fasten one end of this cord at the top of the center spoke C, and the other end of the cord to the next right-hand spoke, 1 foot below the upper end, or in the inner ring; proceed in like manner with every other spoke in succession; and it will be found that, at A, the cord will have the position shown outside the wheel; while at B, C, and D it will also take the respective positions, as shown on the outside. The result in this case will be, that all the weights on the side A, C, D hang to the great or outer circle, while on the side B, C, D all the weights are suspended from the lesser or inner circle. And if we reverse the motion of the wheel, turning it from the left to the right hand, we shall reverse these positions also (the lower end of the cord sliding in a groove towards a left-hand spoke), but without the wheel having any tendency to move of itself."

Now here we have a very convincing account of the performance of the wheel, written by the Marquis himself, and citing a large number of unimpeachable witnesses of the highest social standing, and we have in addition

a very convincing drawing of the wheel (Fig. 2), with a detailed description by the Marquis's biographer. The detailed criticism of the design may be left as an exercise to the ingenuity of the reader. Two points, however, deserve comment. In the first place, it is not claimed

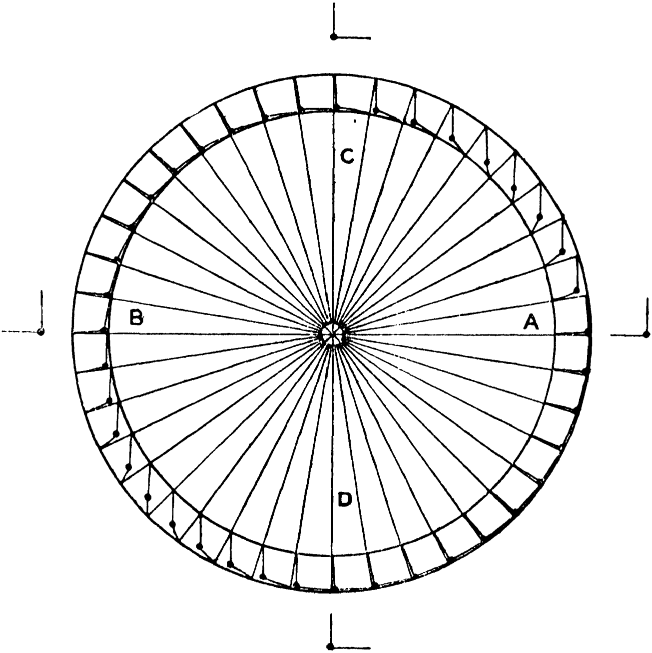

FIG. 2.—Suggested design of Marquis of Worcester's wheel.

that the witnesses ever saw the wheel work. They are cited merely to give evidence as to the way in which the weights distribute themselves. In the second place, when discussing work and energy, we saw that in the case of weights it is vertical height which counts. Whatever path a weight descends by, it loses an amount of

potential energy equal to the product of the weight and the vertical height through which it has fallen, and before it can be restored to its former height it must again acquire this amount of potential energy. Thus there is no balance of energy available to keep the wheel working. The idea that a weight may in passing down one path and rising through another secure a favourable balance of energy is the fallacy which lies at the root of many attempts to solve the problem of perpetual motion, some of which are illustrated in Plate II. We shall see in the next chapter how strong are the reasons for believing this to be impossible. Meantime, the design of the Marquis of Worcester may be criticised in the words of Thomas Young, a scientist of the early nineteenth century, who has been described by Helmholtz as being "next to Leonardo da Vinci the most versatile genius in history." In his *Lectures on Natural Philosophy*, delivered at the Royal Institution in 1801, he makes the following comments: "One of the common fallacies, by which the superficial projectors of machines for obtaining perpetual motion have been deluded, has arisen from imagining that any number of weights ascending by a certain path, on one side of the center of motion, and descending on the other at a greater distance, must cause a constant preponderance on the side of the descent: for this purpose the weights have either been fixed on hinges, which allow them to fall over at a certain point, so as to become more distant from the center, or made to slide or roll along grooves or planes which lead them to a more remote part of the wheel, from whence they return as they ascend; but it will appear on the inspection of such a machine, that although some of the weights are more distant from the center than others, yet there is always a proportionately smaller number of them on that side

on which they have the greatest power, so that these circumstances precisely counterbalance each other."

From the time of the Marquis of Worcester up to the present day there has been an unceasing stream of suggested designs—so much so that the United States Patent Office now insists on a working model as an essential preliminary to the registration of a design. A very full account of the better known attempts to solve the problem will be found in *Perpetuum Mobile*, by Dircks, the biographer of the Marquis of Worcester.

Orffyreus' Device

Where the fame accruing to the discoverer was likely to be so widespread and the money rewards by no means to be despised, it is hardly to be expected that all attempts to solve the problem should be honest ones. One probably dishonest attempt is thus described by Phin in the book already quoted: "The real name of this inventor was Jean Ernest Elie-Bessler, and he is said to have manufactured the name Orffyre by placing his own name between two lines of letters, and picking out alternate letters above and below. He was educated for the Church, but turned his attention to mechanics and became an expert clockmaker. His character, as given by his contemporaries, was fickle, tricky, and irascible. Having devised a scheme for perpetual motion, he constructed several wheels which he claimed to be self-moving. The last one which he made was 12 feet in diameter and 14 inches deep, the material being light fine boards covered with waxed cloth to conceal the mechanism. The axle was 8 inches thick, thus affording abundant space for concealed machinery.

"This wheel was submitted to the Landgrave of Hesse, who had it placed in a room which was then locked,

and the lock secured with the Landgrave's own seal. At the end of forty days it was found to be still running.

"Professor 's-Gravesande having been employed by the Landgrave to make an examination and pronounce upon its merits, he endeavoured to perform his work thoroughly; this so irritated Orffyreus that he broke the machine in pieces, and left on the wall a writing stating that he had been driven to do this by the impertinent curiosity of the Professor!"

Is Perpetual Motion Impossible?

So far, then, the record of attempts to devise a perpetual motion machine is a record of failure. Yet this is in itself no reason for discontinuing the efforts. Many of Nature's most precious secrets have been wrested from her after a long series of unsuccessful efforts. The pathway to success in the solution of any great problem is strewn with previous failures. But there is another and deeper reason for our disbelief in the value of all efforts to solve this particular problem. It is not that we can prove success to be impossible. It is not merely that success has so far not been attained. It is that success would be inconsistent with one of the most far-reaching general principles which science has so far discovered. It is a principle which in its turn cannot be proved to be true. But it so exactly describes and explains such a wide range of phenomena that we are almost compelled to accept it. What this principle is, how it revealed itself to the minds of great inquirers, and what exactly its present bearing is, the sequel will attempt to show.

PLATE I

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55. A double Water-ſcrue, the innermoſt to mount the water, and the outermoſt for it to deſcend more in number of threds, and conſequently in quantity of water, though much ſhorter then the innermoſt ſcrue, by which the water aſcendeth, a moſt extraordinary help for the turning of the ſcrue to make the water riſe.

56. To provide and make that all the Weights of the deſcending ſide of a Wheel

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Wheel ſhall be perpetually further from the Centre, then thoſe of the mounting ſide, and yet equal in number and heft to the one ſide as the other. A moſt incredible thing, if not ſeen, but tried before the late King (of bleſſed memory) in the *Tower*, by my directions, two Extraordinary Embaſſadors accompanying His Majeſty, and the Duke of *Richmond* and Duke *Hamilton*, with moſt of the Court, attending Him.

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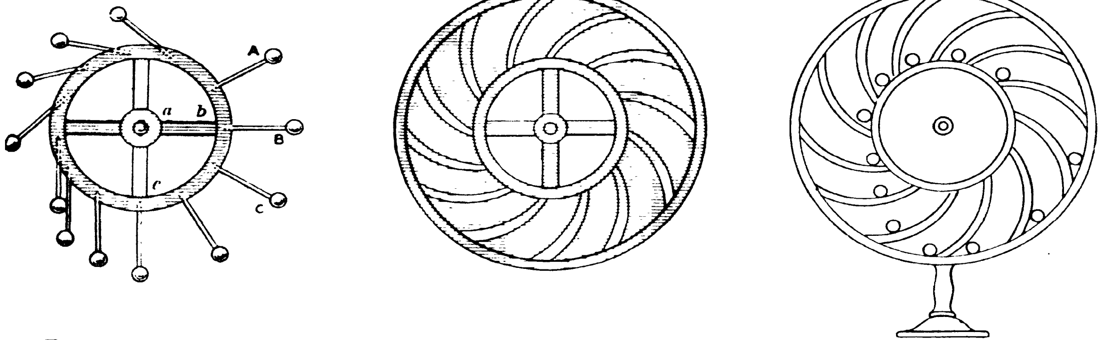
Him. The Wheel was 14. Foot over, and 40. Weights of 50. pounds apiece. Sir *William Balfore*, then Lieutenant of the *Tower*, can juſtifie it, with ſeveral others. They all ſaw, that no ſooner theſe great Weights paſſed the Diameter-line of the lower ſide, but they hung a foot further from the Centre, nor no ſooner paſſed the Diameter-line of the upper ſide, but they hung a foot nearer. Be pleaſed to judge the conſequence. An

Three Pages from "A Century of the Names and Scantlings of Inventions," by the Marquis of Worcester (1663). Paragraph 56 describes his Wheel.

PLATE II



Three Wheels designed to achieve Perpetual Motion: The First depending on Hinged Rods, the Second on Mercury in Curved Tubes, and the Third on Balls rolling in Curved Channels.

CHAPTER III

GROWTH OF THE BELIEF THAT PERPETUAL MOTION IS IMPOSSIBLE

GALILEO

WE must now pass to consider the growth of this conviction that all attempts to invent purely mechanical perpetual motion machines were foredoomed to failure. We find that while some men—and men sometimes of light and leading—were still pursuing this will-o'-the-wisp, others were content to rest their demonstrations of mechanical principles on the impossibility of its attainment. Foremost among these was Galileo—a man who typifies as possibly no other single man does the modern scientific spirit. Originally designed by his father for a career as a cloth merchant, his natural bent for science was so strong that all efforts to deflect him from a scientific career proved unavailing and he had to be allowed to follow his own decided bent. The problem which particularly interested him was the problem of falling bodies. In his day it was universally believed that the speed with which a body falls toward the earth when left to itself is directly proportional to its weight. There is much in a superficial examination of our experience to justify this conclusion. A stone falls much more rapidly than a feather. But this superficial examination of experience can never satisfy the truly scientific mind, and so Galileo devised experiments to test the conclusion. His famous demonstration that all bodies tend to fall with the same speed,

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made by dropping bodies of different weight from the top of the leaning Tower of Pisa, and his explanation of the common departures from this uniform tendency as due to the resistance of the air, are well known and do not come within the scope of our present purpose. In order to follow up this discovery, however, it was necessary that he should carry out experiments to see how the speed of the falling body increased with the time during which it had been falling, and how it increased with the distance fallen. So great is the speed of falling bodies and so accurate the timing required that direct experiments are hardly possible even in a modern well-equipped laboratory. To Galileo, with no stop-watch, and indeed with no watch of any kind, the difficulties were such as would have discouraged most men. Abandoning the attempt to measure the speed of falling bodies directly, he looked round for a method of " diluting " gravity so as to make it less strong and give him speeds which he could measure. It occurred to him that gravity was the force which made a ball roll down an inclined plane, and here, by adjusting the slope of the plane, he could make the motion almost as slow as he liked. But this " dilution " of gravity could only be justified if the law of descent down an inclined plane was the same as that of a freely falling body. He therefore set himself to prove that the velocity acquired by a body depended only on the vertical height through which it had fallen, and was

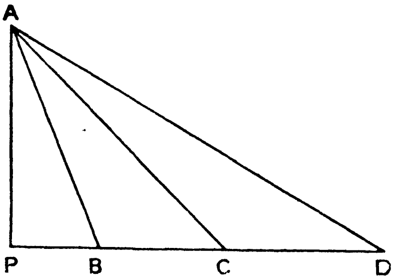


FIG. 3.—The fall of a body down inclined planes of different slope.

independent of the slope of the plane down which it had come, so that if we take a series of inclined planes AB, AC, AD, etc. (Fig. 3), the velocity at B, C, and D will be the same as if the body had fallen freely through the height AP. This assumption he proceeded to justify by two methods, both of which are of the very greatest interest from our present point of view. His first method was a theoretical one. If it is not true, let us consider two planes of different slope RS and TS, both of the same height (Fig. 4); and let the velocity acquired in coming down the plane RS be greater than that acquired in coming down the plane TS. Then the velocity lost in ascending the plane TS will be less than that acquired in falling down the plane RS. Start a body at R and when it falls to S deflect it so that it rises up ST. When it reaches T it will not have lost all its velocity. It will therefore rise higher than T—*i.e.* it will rise to a point higher than that from which it started. This, Galileo maintains, is impossible. Why? Simply because it would give us a very simple perpetual motion machine. The body could then be allowed to drop back to its original level, working a wheel as it dropped, and the whole cycle could be repeated. No formal proof that it cannot happen is possible, yet Galileo is satisfied. In order to remove all possibility of doubt, however, Galileo confirms his assumption by a very simple and beautiful experimental proof which he himself thus describes:

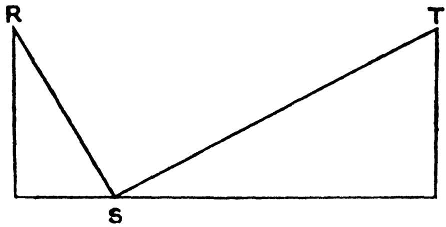


FIG. 4.—Perpetual motion by combining two inclined planes.

"Suppose this sheet of paper (Fig. 5) to be a vertical wall, and from a nail driven in it a ball of lead weighing 2 or 3 ounces to hang by a very fine thread AB, 4 or 5 feet long. On the wall mark a horizontal line DC, perpendicular to the vertical AB, which latter ought to hang about 2 inches from the wall. If now the thread AB with the ball attached take the position AC and the ball be let go, you will see the ball first descend through the arc CB and passing beyond B rise through the arc BD almost to the level of the line CD, being prevented from reaching it exactly by the resistance of the air and of the thread. From this we may truly conclude that its impetus at the point B, acquired by its descent through the arc CB, is sufficient to urge it through a similar arc BD to the same height. Having performed this experiment and repeated it several times, let us drive in the wall, in the projection of the vertical AB, as at E or F, a nail 5 or 6 inches long, so that the thread AC, carrying as before the ball through the arc CB, at the moment it reaches the position AB, shall strike the nail E and the ball be thus compelled to move up the arc BG described about E as centre. Then we shall see what the same impetus will here accomplish, acquired now as before at the same point B, which then drove the same moving body through the arc BD to the height of the horizontal CD. Now, gentlemen, you will be pleased to see the ball rise to the horizontal line at the

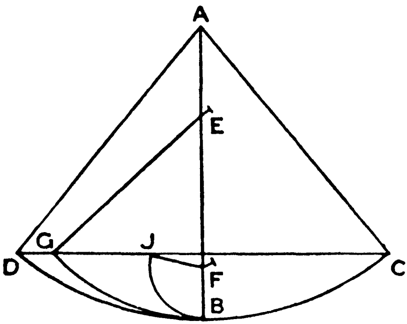


FIG. 5.—Galileo's pendulum experiment.

point G, and the same thing also happen if the nail be placed lower as at F, in which case the ball would describe the arc BJ, always terminating its ascent precisely at the line CD."

The passage from circular arcs of different curvature to inclined planes of different slope is a fairly obvious one, and Galileo rightly concludes that "this experiment leaves no room for doubt as to the truth of the supposition." But if whatever path a heavy body describes it has only energy enough to bring itself back to the height from which it started, then obviously it cannot have any energy to give up for a turning wheel, and the Marquis of Worcester and all who made similar attempts to his might have spared themselves much expenditure of time and ingenuity.

Stevinus

And now let us look into the mind of another natural philosopher—Stevinus of Bruges (1548–1620). Stevinus is interested in the problem of the inclined plane considered as a machine. As we have already seen, a machine is a contrivance which enables us to overcome a large resistance by the application of a small force. The inclined plane is a primitive form of machine. We can drag a body up an incline—or roll it up, as is often done with barrels of beer—when it is too heavy for us to lift. Now the important thing about a machine is, what resistance can we overcome by the application of a given force? Suppose we had a perfectly smooth inclined plane, what weight could we push up it by the use of a given force. If we call the force used the "effort," then the ratio of the weight to the effort is what is known as the "mechanical advantage" of the machine. It follows that if the mechanical advantage is 8, then by exerting

any given effort we can, with the help of the machine, overcome a resistance 8 times as great.

The argument of Stevinus is in effect this : Consider a smooth inclined plane ABC, the base AC being horizontal (Fig. 6). On this plane sling an endless cord to which, at equal distances apart, a number of balls—thirteen in this case—are attached at equal intervals.

Now there are seven balls pulling to the left of B, and only five to the right. Yet Stevinus boldly asserts that the arrangement is in equilibrium. For if the seven on the right are heavier than the five on the left, motion will result. " But if this took place, our row or ring of balls would come once more into their original position " (when each ball had succeeded the one in front in position), " and from the same cause the seven globes to the left would again be heavier than the five to the right, and therefore these seven would sink a second time and those five rise, and all the globes would keep up, of themselves, a continuous and unending motion which is false." And this is written in 1605 ! And it leads Stevinus to the correct solution of the problem of the inclined plane. The solution is such an artistic one that although there are now other and more obvious ways of deriving it, we shall follow Stevinus as he works it out. The seven

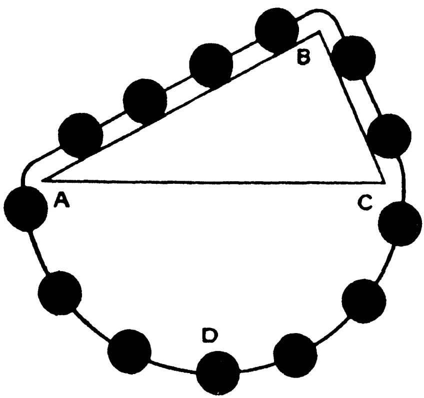


FIG. 6.—Stevinus' solution of the problem of the inclined plane.

balls in the part ADC are symmetrically placed, and therefore pull equally on A and on C respectively, and can therefore be removed without affecting the problem. This leaves us with four balls on AB and two on AC, and these balance. To make the solution more obvious we may replace Stevinus' balls by a uniform chain, and we see that a weight of chain proportional to the length of AB and lying on it exactly balances a weight of chain proportional to BC even if BC be now made vertical. Replace the chain by two weights W and P, attached by a string, and we see that a weight W lying on the plane can be held in position by a string parallel to the plane to which a force equal to the weight of P is applied. Now the mechanical advantage = ratio of weight to effort $=\frac{W}{P}=\frac{AB}{BC}$ by Stevinus' reasoning. The mechanical advantage of an inclined plane is thus the ratio of its length measured up the slope to its vertical height.

In his chapter on Hydrostatics in the same work, we find Stevinus basing his arguments on a similar proposition. One of his fundamental principles is that a given mass of water preserves within water its given place. His demonstration is as follows (Fig. 7):

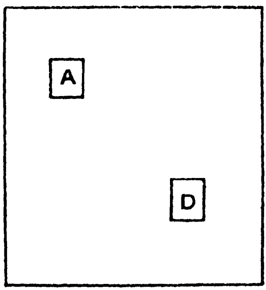


FIG. 7.—Stevinus' principle of hydrostatics.

"For, assuming it to be possible by natural means, let us suppose that A does not preserve the place assigned to it, but sinks down to D. This being posited, the water which succeeds A will, for the same reason, also flow down to D; A will be forced out of its place in D; and thus this body of water,

for the conditions in it are everywhere the same, will get up a perpetual motion, which is absurd." From this all the principles of hydrostatics are deduced.

Conservation of Mechanical Energy

Similar quotations might be cited from Leonardo da Vinci, Huygens, and others, but enough has been written to indicate an interesting situation. On the one hand, we have a fruitless and unceasing quest for a perpetual motion machine ; on the other, we have a succession of the greatest minds developing the science of mechanics from the assumed impossibility of perpetual motion, and even making this assumed impossibility a method of discovery of new truth. Perhaps enough has also been written to suggest a relevant question. If the impossibility of perpetual motion underlies so much that is important, why do we so rarely hear of it to-day ? If it led to important discoveries in the past, why has it ceased to appear in this rôle ?

As a matter of fact, the principle is still with us. Its importance is as great as ever. But it has now become part of one of the greatest generalisations of modern physics—the law of conservation of energy. Restricting this principle for the time being to the mechanical sphere and excluding heat, electricity, magnetism, etc., we may say that there is always a definite equivalence between kinetic energy, potential energy, and work. Work can never be done without the disappearance or using up of an equivalent amount of energy. It follows that since motion is always resisted by some kind of friction, and since the overcoming of resistance is work, therefore motion is impossible without the expenditure of energy. In winding up the eight-day clock you do a certain amount of work. This work (neglecting what is spent in over-

coming friction) is stored up as potential energy of the weights, and the weights in falling lose this potential energy and do an exactly equivalent amount of work on the clockwork. In winding up the clock you do exactly as much work as if you had stood by the clock for eight days and turned the clockwork by hand. Doing the work rapidly and storing it up as potential energy in the weights is a mere matter of convenience. So for the watch, in coiling the mainspring you are storing energy, and the energy which keeps the watch going is derived from you, having been stored up meanwhile in the distorted mainspring.

When the "monkey" of a pile-driver is raised to a given height by an engine, work is done on it, and this work is stored up in it as potential energy. In falling, the monkey loses potential energy but acquires kinetic energy in its place, and if there is no loss this kinetic energy is the exact equivalent of the work done on it by the engine. When the monkey strikes the pile it loses its kinetic energy, but work is done in driving in the pile. This work is the exact equivalent (if there is no loss) of the kinetic energy of the monkey and is identical with the amount of work originally done by the engine in raising the monkey.

By no mechanical device can we ever get work done without the expenditure of its due equivalent of energy. We cannot be too clear about this important principle. At first sight machines like the lever or a system of pulleys may seem to evade it. An examination candidate once defined a machine as a device whereby a large amount of work could be done with the expenditure of a small amount of energy. The discoverer of that type of machine will have achieved perpetual motion and will revolutionise our civilisation. A machine does enable us to overcome a large resistance by the application of a

small effort, but it will always be found that this effort has to be expended over a considerably larger distance, and in the most favourable possible case—an entirely frictionless machine—the work done by the machine is always exactly equal to the work done on it—*i.e.* to the energy it absorbs. In no actual case can friction be eliminated, and therefore in no actual case can all the energy absorbed by the machine make its appearance as useful work.

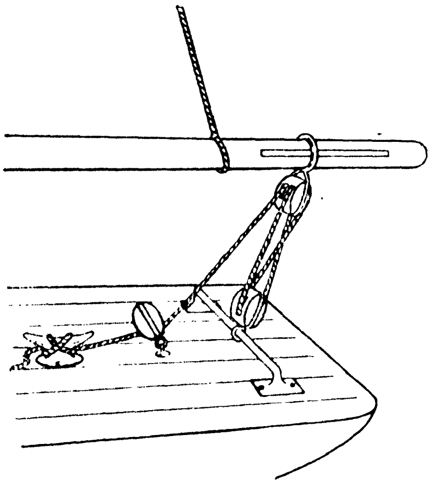


FIG. 8.—Mainsheet of a yacht.

These two factors—the increased distance through which the effort must act and the absorption of energy in overcoming friction—always set a limit to the mechanical advantage which can usefully be employed in any actual case. The boom of a yacht is adjusted in position by a rope known as the mainsheet, which passes through a number of pulleys attached to the boom and a corresponding number attached to the deck. The force required to haul in the boom or hold it in position will depend on the number of pulleys. The greater the number of pulleys the less the effort required to haul in the boom when desired, but the greater will be the length of sheet which must be pulled through to give any desired movement to the boom. Also the greater the number of pulleys used, the greater the amount of friction which must

be overcome, and therefore the greater the amount of useless work we must do. So far from giving us any gain of energy, the work we have to do is greater than that required to haul in the boom directly (supposing our strength to be equal to that task) by the amount of work necessary to work the pulleys round against friction.

In asserting this principle of the conservation of mechanical energy we deny the possibility of a mechanical perpetual motion, and although the principle is, from its very nature, incapable of direct proof, it is one of the most firmly established principles of modern physics.

CHAPTER IV

EXTENSION OF THE PRINCIPLE TO OTHER FORMS OF ENERGY

Nature of Heat

So far we have confined our attention strictly to mechanical contrivances and machines. We have next to consider whether this principle of the impossibility of perpetual motion—this law of conservation of energy—will still hold when we remove this restriction. If we are allowed free scope to bring in heat, electricity, magnetism, chemical action, is the quest for perpetual motion still hopeless, or is there any chance that we may be able to manipulate these new quantities in such a way that we may hope to be able "to perform a large amount of work with the expenditure of a small amount of energy"? We know that in the steam-engine the passage of heat from the fire to the boiler is the first step in a series of events which enables us to overcome great resistances, and that in the electric motor the passage through the apparatus of an electric current results in the performance of a large amount of mechanical work.

Historically the relations between heat and work were the first to be investigated—no doubt on account of the greater familiarity of the phenomena associated with heat. As a result of these investigations not only were the relations between heat and work completely elucidated, but this elucidation also furnished the key to an unsolved problem which we have already passed over without

comment. Why is it that in mechanical machines we always have to safeguard our statements of the law of conservation of energy by supposing our machines to be frictionless ? In actual practice not only is perpetual motion impossible, but experiment shows that we always get out of an actual machine less energy than we put into it. What becomes of this lost energy ? What becomes of energy used up simply in overcoming friction ? Suspend a massive ball by a long thread and displace the ball to one side. In so doing you raise it a little above the level of its undisplaced position, do a little work on it and give it energy of position—potential energy. Release it and it swings towards its mean position, losing potential energy and gaining kinetic energy. When it reaches its mean position all its energy is kinetic, and it now begins to rise on the opposite side, losing kinetic energy and gaining potential energy. If the pendulum is left to itself this interchange of the two kinds of energy goes on for a long time. If we could remove all resistance to its motion the pendulum would, when once started, presumably go on swinging for ever. In practice, however, the swings gradually die away—the mechanical energy entirely disappears. The kinetic energy acquired in the first quarter-swing of the pendulum to its undisplaced position was not quite equal to the potential energy originally given to it. This kinetic energy in turn was not wholly converted into potential energy in the second quarter of the swing. When the swing has been completed, the pendulum bob will not have regained its original displaced position. All the time there is a steady loss of mechanical energy. What becomes of it ?

When I haul in the mainsheet of a yacht the work I do on the sheet is always greater than the work actually done on the boom. What becomes of the energy I have uselessly expended ?

Answers to these questions were only found as the result of much careful and painstaking research, and we shall now review shortly the course which this research took.

The Caloric Theory

Up to the end of the eighteenth century it was supposed that heat was an imponderable fluid held in the pores of all substances. This was the popular "theory" of the nature of heat. The fluid was called "caloric," and the theory was known as the "caloric theory." Now a scientific theory is a guiding idea with which we approach the known facts and into which we endeavour to fit them. If they fit, the guiding idea connects them all up, gives them meaning and, as we say, "explains" them. It makes the facts easily remembered and easily communicated to any one to whom we may wish to teach them. As new facts accumulate, each must in turn be fitted in to its place in the theory. If it cannot be made to fit then we become dissatisfied with our theory and look about for one more comprehensive to replace it. Sometimes the theory will suggest new facts, hitherto undiscovered, for which we ought to search. If we find them, the position of our theory is much strengthened.

Now the value, growth, and final supersession of a scientific theory is particularly well illustrated by the caloric theory. This theory was a very useful guiding idea for the known facts of heat in the eighteenth century. When two bodies at different temperatures are placed in contact, the hot one gets colder and the cold one hotter. This is due to the caloric fluid passing from the hot body to the cold one. The two bodies finally attain the same temperature. Temperature may be regarded as the "level" of the caloric, and just as water in two vessels connected by a tube will flow from one to the other until

the levels in the two are equal, so the caloric flows until the temperatures are equal. When a piece of metal is hammered it grows hot. The fluid is being hammered out of its pores. When a piece of metal is bored it gets hot. This is because the borings being in small pieces cannot contain so much caloric, and it gets pressed out. So far all is beautifully simple and consistent. We feel, as the scientists of that time felt, that it must be " true." One fact is a little more obstinate than the others. When a hot body is placed in contact with ice the hot body loses caloric but the ice does not get warmer. It forms water at the same temperature as the ice. A little manipulation, however, will serve to make this fact fit. We have only to suppose that the caloric combines with the ice to form water, and in so doing ceases to be sensible. Thus water is a compound of ice and caloric.

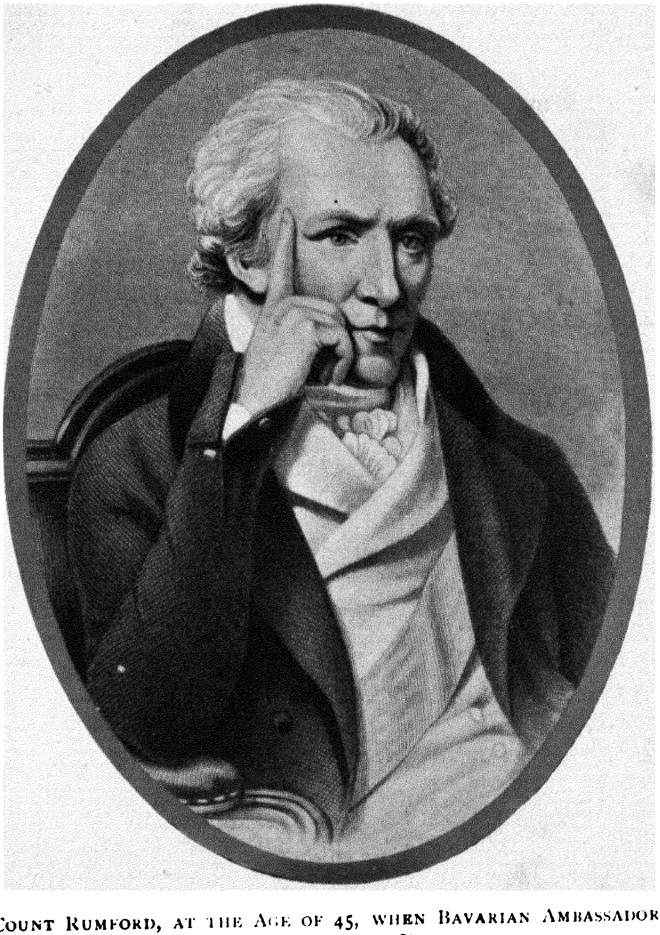
The Kinetic Theory

Alongside of this prevailing view of the nature of heat, another, born of the desire for simplicity, had already begun to suggest itself. The laws of mechanics being very completely elucidated, might it not be that all the phenomena of physics—heat, light, magnetism, electricity, chemical action—should all be explained in terms of mechanics? Might not all these phenomena be due to the movements of very small particles, and might not they all be explained by the velocities, energies, and attractions of these minute masses? Huygens (*Traité de la lumière*, 1690) throws out the suggestion in the following words: " There can be no doubt that light consists of the *motion* of a certain substance. For if we examine its production, we find that here on earth it is principally fire and flame which engender it, both of which contain beyond doubt bodies which are in rapid

movement, since they dissolve and destroy many other bodies more solid than they; while if we regard its effects, we see that when light is accumulated, say by concave mirrors, it has the property of combustion just as fire has—that is to say, it disunites the parts of bodies, which is assuredly a proof of *motion*, at least in the *true philosophy*, in which the causes of all natural effects are conceived as *mechanical* causes. Which in my judgment must be accomplished or all hope of ever understanding physics renounced." It is easy to criticise this view of Huygens as an explanation of the phenomena he adduces, but the instinct to reduce all the phenomena of physics to the comparatively simple terms of mechanics was a right and, indeed, an inevitable one. It carried Laplace in a great flight of fancy to a time in which even thought and human activity would be similarly explained. "A mind to which were given for a single instant all the forces of nature and the mutual positions of all its masses, if it were otherwise powerful enough to subject these problems to analysis, could grasp, with a single formula, the motions of the largest masses as well as of the smallest atoms; nothing would be uncertain for it; the future and the past would lie revealed before its eyes" (*Essai philosophique sur les probabilités*, 1840). On this view heat is not an imponderable fluid. It consists in the very rapid motions of the small invisible particles of which all bodies are made up. The more rapid and extensive these motions, the hotter the body.

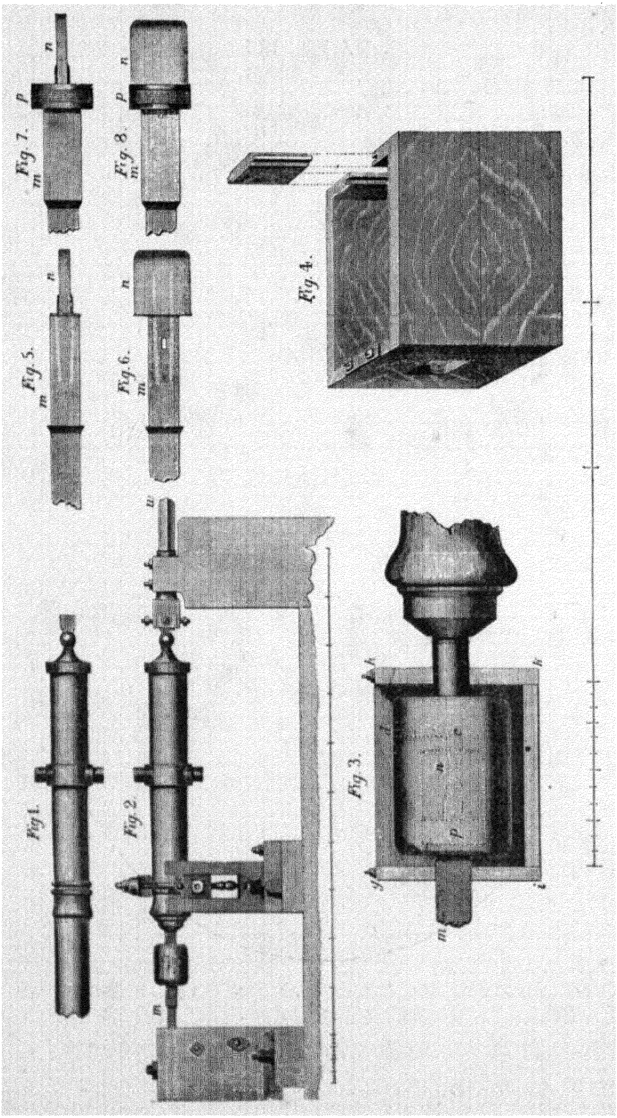
Now this conflict between the two theories of heat has a very direct bearing on our own problem. If all phenomena are at bottom merely mechanical, then presumably the same laws will be found to hold. And if perpetual motion is impossible for mechanical systems, it seems unlikely that we shall be any better off if we try to bring in heat, electricity, and the other natural forces. The two

PLATE III



COUNT RUMFORD, AT THE AGE OF 45, WHEN BAVARIAN AMBASSADOR TO ENGLAND (1798).

PLATE IV



APPARATUS USED BY COUNT RUMFORD TO ADAPT HIS CANNON-BORING OPERATIONS TO THE STUDY OF THE SOURCE OF FRICTIONAL HEAT.

theories are compared by Lavoisier and Laplace (*Mémoire sur la Chaleur*, 1780). " Other physicists think that heat is nothing but the result of the insensible vibrations of matter. In the system we are now examining, heat is the *vis viva* resulting from the insensible movements of the molecules of a body ; it is the sum of the products of the mass of each molecule by the square of its velocity. . . . We shall not decide between the two preceding hypotheses ; several phenomena seem to support the last mentioned—for instance, that of the heat produced by the friction of two solid bodies. But there are others which are more simply explained by the first, and perhaps they both operate at once."

It is, as they rightly imply, a matter of which view more simply and more adequately fits all the facts. At this point it seems as if the caloric theory were superior in its direct application to the phenomena of heat, while the kinetic theory appears to be philosophically the more attractive and satisfactory.

CHAPTER V

DEVELOPMENT OF MECHANICAL THEORY OF HEAT

RUMFORD

THE next important contribution to the subject was made by Benjamin Thompson, Count Rumford (Plate III.), in a paper published in 1798. In this paper he incidentally emphasises the importance of keen observation and thoughtful contemplation of the ordinary and the commonplace.

"It frequently happens that, in the ordinary affairs and occupations of life, opportunities present themselves of contemplating some of the most curious operations of nature; and very interesting philosophical experiments might often be made, almost without trouble or expense, by means of machinery contrived for the mere mechanical purposes of the arts and manufactures.

"I have frequently had occasion to make this observation; and am persuaded, that a habit of keeping the eyes open to everything that is going on in the ordinary course of business life has oftener led, as it were by accident, or in the playful excursions of the imagination, put into action by contemplating the most common appearances, to useful doubts and sensible schemes for investigation and improvement, than all the more intense meditations of philosophers, in the hours expressly set apart for study."

If the importance of this attitude of mind requires any further emphasis, it is only necessary to look at the titles

of the scientific papers of a master like Stokes, or Rayleigh, or Kelvin. The waves produced by a ship, the spinning of a top, the colour of a butterfly's wing, the fall of a drop of water, all these suggest to the master mind the unsolved mysteries of the commonplace to which familiarity has blinded us.

RUMFORD'S EXPERIMENT

Following out his own precept, he proceeds to adapt his cannon-boring mechanism to the needs of his experiment. Plate IV. shows Rumford's apparatus, which he describes in the following terms :

" Fig. 1 shews the cannon used in the foregoing experiments in the state it was in when it came from the foundry.

" Fig. 2 shews the machinery used in the experiments No. 1 and No. 2. The cannon is seen fixed in the machine used for boring cannon.

" *w* is a strong iron bar, which bar, being united with machinery that is carried round by horses, causes the cannon to turn round its axis.

" *m* is a strong iron bar, to the end of which the blunt borer is fixed ; which, by being forced against the bottom of the bore of the short hollow cylinder that remains connected by a small cylindrical neck to the end of the cannon, is used in generating Heat by friction.

" Fig. 3 shews, on an enlarged scale, the same hollow cylinder that is represented on a smaller scale in the foregoing figure. It is here seen connected with the wooden box (*g*, *h*, *i*, *k*) used in the experiments No. 3 and No. 4, when this hollow cylinder was immersed in water.

" *p*, which is marked by dotted lines, is the piston which closed the end of the bore of the cylinder.

" *n* is the blunt borer seen sidewise.

" *d*, *e*, is the small hole by which the thermometer was introduced that was used for ascertaining the Heat of the cylinder. To save room in the drawing, the cannon is represented broken off near its muzzle; and the iron bar to which the blunt borer is fixed is represented broken off at *m*.

" Fig. 4 is a perspective view of the wooden box, a section of which is seen in the foregoing figure. (See *g*, *h*, *i*, *k*, Fig. 3.)

" Figs. 5 and 6 represent the blunt borer *n*, joined to the iron bar *m*, to which it was fastened.

" Figs. 7 and 8 represent the same borer, with its iron bar, together with the piston which, in the experiments No. 2 and No. 3, was used to close the mouth of the hollow cylinder."

Rumford then proceeds to discuss his results as follows:

" Being engaged, lately, in superintending the boring of cannon, in the workshops of the military arsenal at Munich, I was struck with the very considerable degree of Heat which a brass gun acquires, in a short time, in being bored; and with the still more intense heat (much greater than that of boiling water, as I found by experiment) of the metallic chips separated from it by the borer.

" The more I meditated on these phenomena, the more they appeared to me to be curious and interesting. A thorough investigation of them seemed even to bid fair to give a further insight into the hidden nature of Heat; and to enable us to form some reasonable conjectures respecting the existence, or non-existence, of an *igneous fluid*—a subject on which the opinions of philosophers have, in all ages, been much divided.

" From *whence comes* the Heat actually produced in the mechanical operation above mentioned?

"Is it furnished by the metallic chips which are separated by the borer from the solid mass of metal?

"If this were the case, then, according to the modern doctrines of latent Heat, and of caloric, the *capacity for Heat* of the parts of the metal, so reduced to chips, ought not only to be changed, but the change undergone by them should be sufficiently great to account for *all* the Heat produced."

Here Rumford sees a consideration which will undoubtedly have an important bearing on the struggle between the rival theories. If the borings have really had all this caloric pressed out of them they must contain much less caloric than the unabraded metal. They might therefore be expected to show a greater capacity for heat—*i.e.* we might reasonably expect them to require more heat to raise them one degree in temperature. Rumford found no trace of any effect of this kind, but unfortunately his experiments were not very convincing. Had his method been more satisfactory and had he still found no trace of the effect he was seeking, the caloric theory would have had to be so modified to fit this new fact that the kinetic theory would have obtained an advantage.

His paper shows him to have been a thoughtful and careful experimenter, alive to the possible sources of error and painstaking in avoiding them or allowing for them. By preventing as far as possible all loss of heat, he was able to boil 19 pounds of water and to heat the castings of the cannon by the work of two horses for two hours and twenty minutes.

In his account of this novel experiment, he says: "It would be difficult to describe the surprise and astonishment expressed in the countenances of the bystanders, on seeing so large a quantity of cold water heated, and actually made to boil, without any fire.

"Though there was, in fact, nothing that could be

justly considered as surprising in this event, yet I acknowledge fairly that it afforded me a degree of childish pleasure which, were I ambitious of the reputation of a *grave philosopher*, I ought most certainly rather to hide than to discover."

He sums up his reasoning in these words: "In reasoning on this subject, we must not forget to consider that most remarkable circumstance, that the source of the Heat generated by friction in these experiments, appeared evidently to be *inexhaustible*. It is hardly necessary to add, that anything which any *insulated* body or system of bodies can continue to furnish *without limitation*, cannot possibly be *a material substance*. It appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in the manner in which the heat was excited and communicated in these experiments, except it be motion. I am very far from pretending to know how or by what means or mechanical contrivance that particular kind of motion in bodies which is supposed to constitute Heat is excited, continued, and propagated."

Rumford's Conclusion

Here, then, is an experiment which does not fit in very well with the caloric theory—is, in fact, extremely difficult to reconcile with it at all. It is not yet sufficient to justify the abandonment of the caloric theory. It does very strongly suggest, however, that somehow the work done by the horse is appearing in a new form as heat. Not only so, but Rumford supplies us with so much detailed information about his experiments that we can, as we shall see later, make a rough estimate of the amount of work which must be expended in order to produce a given amount of heat.

Davy

In the year following the experiments of Rumford, Sir Humphry Davy performed another experiment which was harder still to reconcile with the caloric theory. He showed that if two pieces of ice were protected from all sources of heat and kept rubbing together, then the ice melted to form water. Now the ice must have combined with caloric to produce the water. Where did this caloric come from? We can now see that this experiment is a very convincing modification of Rumford's experiment. Work is being done in keeping the pieces of ice in motion against the friction at their common surface. The great advantage is that instead of producing borings from a cannon—borings which *may* contain less heat than the metal from which they come—we are here producing water from ice, and by the assumptions of the caloric theory itself the water contains *more* heat than the ice from which it comes. If Davy had only pushed home his advantage, great discoveries awaited him. Unfortunately he only half realised the significance of his own contribution, and although in 1812 he did go the length of saying "The immediate cause of the phenomenon of heat, then, is motion, and the laws of its communication are precisely the same as the laws of the communication of motion," he never followed the matter up. Neither, indeed, did any one else for some thirty years afterwards.

Carnot

The next contribution to the subject was made by a young French engineer—Sadi Carnot—in the year 1824, in a paper called *Le Puissant Motrice de Feu*. The invention of the steam-engine had given men's minds a strong bent towards the investigation of the relation

between heat and work. The point from which Carnot started is made quite clear in his book: "The motive-power of a fall of water depends on its height and on the quantity of liquid; the motive-power of heat depends also on the quantity of caloric employed and on what might be called—in fact what we shall call—the height of fall—that is to say, the difference in temperature of the bodies between which the exchange of caloric takes place." In other words, just as by allowing water to be taken in at a high level and discharged at a lower level—as in the overshot water-wheel—we can make it perform useful work, so by taking in heat at a high temperature to the boiler of a steam-engine and discharging it at a lower temperature to the condenser, we can obtain work from it. There are two points in this statement which deserve special mention. In the first place, it is based on the caloric theory. There is no suggestion of any loss of heat in the process. The work is done merely by the fall of a given quantity of caloric through a given range of temperature. In the second place, the energy made available, the amount of work which can be done, depends only on the quantity of heat passed through the engine, and on the temperatures of the boiler and condenser. It will be the same for a steam-engine as for one using air or any other gas or vapour. Carnot expresses this with great clearness: "The motive-power of heat is independent of the agents brought into play for its realisation, and its quantity is fixed solely by the temperature of the bodies between which, in the last resort, the transfer of caloric is effected."

CARNOT'S CYCLE

In justifying this proposition, Carnot made use of what has been one of the most fruitful conceptions in the history of physics since his time—the reversible cycle

of operations. This conception involves first of all the idea of a *cycle*. It is not enough to give a certain quantity of heat to a vapour, allow it to expand against a resistance and so do work, and then proceed to find the relation between the heat supplied and the work done. The vapour is left in a state different from that in which it started. The cycle must be completed by bringing it back to its old volume and temperature before we can reason about any relation between the heat given to it and the work done. His conception also involves the idea of *reversibility*. If the letting down of a given quantity of heat from one temperature to another by the performance of a cycle results in a given quantity of work, then if the engine is reversible the performance of that same quantity of work in driving the engine backwards would raise the given quantity of heat through the same range of temperature, leaving everything as before. A reversible heat engine would thus be exactly analogous to a frictionless overshot water-wheel which spilt no water. In carrying a certain quantity of water from the level above to the level below, the wheel would acquire the energy for a certain amount of work. If the wheel were now worked backwards it would lift water from the lower to the higher level, but in order to return the same amount of water would require the expenditure of the same amount of work. Suppose now, that following Carnot's reasoning we have two reversible engines A and B both working between the same temperatures. If A transfer Q units of heat from the higher to the lower temperature, it does an amount of work W. While when B transfers the same amount of heat, it does an amount of work $W + W'$. Now use the engine B to drive the engine A backwards. B takes Q units of heat from the high temperature to the low temperature, and is capable of doing $W + W'$ units of work in consequence.

Use W units to work A backwards, and it will restore Q units of heat from the low temperature to the high temperature again. All will be as before except that we have gained W′ units of work from nowhere. This, Carnot argues, is impossible. Why? Because even if heat is called in to the process, a perpetual motion is impossible. This is the first assertion of a universal impossibility of perpetual motion. And if it is impossible, what then? Why, all reversible engines are *equally* efficient whatever process they use and whatever material they work with. Here there opens an attractive vista of possibilities, but the developments of this principle would take us too far from our present purpose.

All this work of Carnot remained almost unnoticed. It was reserved for a British physicist—Lord Kelvin—to bring him to the notice he deserved. As a result of subsequent investigation it became apparent that the value of his work was completely independent of the theory of heat which he held, and that his conclusions were just as valid for the kinetic theory as for the caloric theory.

The honour of having established the mechanical nature of heat upon a firm foundation belongs unquestionably to James Prescott Joule, but before we go on to learn something of him as a man and as a scientific worker, reference must be made to the contributions of two of his immediate precursors, Séguin and Mayer.

Séguin and Mayer

If a compressed gas is allowed to expand, it loses heat and can be made to do work in expanding. If a gas is compressed (as in pumping up a bicycle tyre), work is done on it and it is warmed. Now both Séguin and Mayer had some idea that work and heat are equivalent.

Séguin assumed that the work done by the expanding gas was the equivalent of the heat it lost. Mayer assumed that the heat which is produced by compressing a gas is the equivalent of the work done in the compression. Neither of them, however, brought the gas back to its initial condition as Carnot showed ought to be done, and neither made experiments to see how far this affected their results.

Séguin worked with the steam-engine and tried to measure the heat taken from the boiler and the heat given to the condenser. Had his experimental methods been adequate he would have found, even after every allowance had been made for the escape of heat, that when the engine was working it always took more heat from the boiler than it gave to the condenser. This would have been a very awkward discovery for the caloric theory.

If we define, as Joule did, the mechanical equivalent of heat as the amount of work which, completely converted into heat, would give one unit of heat, and assume, as Joule proved, that heat and work are really mutually convertible, we can calculate values of this mechanical equivalent of heat both from the work of Séguin and from that of Mayer. In the first case the result was much too high, in the second case it was too low. It is interesting to anticipate here so far as to say that Joule himself performed the experiments necessary to justify Mayer's method, and with more correct data obtained a result in good agreement with the generally accepted value.

His comment on the work of Séguin and Mayer is as follows: "Séguin gives data from which the mechanical equivalent of heat may be readily deduced on his hypothesis, the result being too great in consequence of the thermal effect of the compression of vapour being under-

stated. Neither in Séguin's writings of 1839 nor in Mayer's paper of 1842 were there such proofs of the hypothesis advanced as were sufficient to cause it to be admitted into science without further inquiry. Mayer appears to have hastened to publish his views for the express purpose of securing priority. He did not wait until he had the opportunity of supporting them by facts. My course, on the contrary, was to publish only such theories as I had established by experiments calculated to commend them to the scientific public, being well convinced of the truth of Sir J. Herschel's remark, that *hasty generalisation is the bane of science.*"

PART II.—ACCOUNT OF JOULE'S LIFE AND WORK

CHAPTER VI

LIFE AND WORK OF JOULE

Early Experiments

James Prescott Joule was born in Salford on Christmas Eve, 1818. As a boy he was rather delicate, and was taught at home till the age of sixteen. In 1835 he went with his brother to study under John Dalton, the great chemist, who was then President of the Manchester Literary and Philosophical Society. From Dalton Joule learned geometry and chemistry, but it is probable that the most important result of their association was an inspiration towards original experimental research which seems to date from this time. Provision had to be made for this new activity, and a room in his father's house was set apart as a laboratory. Here he started work on electro-magnets, the first outcome of which was a letter headed, "Description of an Electromagnetic Engine," which appeared in Sturgeon's *Annals of Electricity*, and was dated 8th January 1838—a few days after his nineteenth birthday. This was the first of a series of about one hundred original communications which Joule made in the course of his career to various scientific societies and periodicals—to say nothing of the important joint papers which he published in collaboration with Kelvin and others.

His early work had a bearing curiously indirect on the

main problem to which almost all his mature work was devoted, and it is extremely interesting to follow the path along which he is led to the main issue. Starting with the electromagnetic engine—*i.e.* a motor—he soon appreciated the fact that the performance of the motor depended on (1) the resistance it would overcome, and (2) the speed at which it could be driven. In a second letter to the editor of the *Annals of Electricity*, written earlier than the first but published later, he measures the power of his motor by the rate at which it will lift a weight. "The greatest power I have been able to develop was to raise 15 pounds a foot high in one minute." He does not at first realise where this energy comes from, and in consequence almost hints at the possibility of a perpetual motion. Thus in a further letter, dated 28th May 1839, we find: "I can hardly doubt that electromagnetism will ultimately be substituted for steam to propel machinery. If the power of the engine is in proportion to the attractive force of its magnets, and if this attraction is as the square of the electric force, the economy will be in the direct ratio of the quantity of electricity, and the cost of working the engine may be reduced *ad infinitum*." With characteristic scientific caution, however, he adds: "It is, however, yet to be determined how far the effects of magnetic machinery may disappoint these expectations."

Units

In a paper published in August 1840 he brings before us a very serious difficulty. In the infancy of a science methods of measurement are rough, and the need of accurately defined units is not felt. Electricity was at this time in that stage. Henry Cavendish measured electric discharges by the physiological effect on his own body. In the early days of the Cavendish Laboratory

at Cambridge, when Clerk Maxwell was preparing the papers of Cavendish for publication, it is said that many distinguished visitors were compelled to place their two hands simultaneously in two bowls of water at different electrical potentials and guess the magnitude of the discharge from their sensations. Joule says: "The great difficulty, if not the impossibility, of understanding experiments and comparing them with one another, arises in general from incomplete descriptions of apparatus and from the arbitrary and vague numbers which are used to characterise electric currents. Such a practice might be tolerated in the infancy of the science; but in its present state of advancement greater precision and propriety are imperatively demanded. I have therefore determined for my own part to abandon my old quantity numbers, and to express my results on the basis of a unit which shall be at once scientific and convenient. . . . 1. A degree of static electricity is that quantity which is just able to decompose 9 grains of water. 2. A degree of current electricity is the same amount propagated during each hour of time."

Duty of an Electric Motor

By 1841 he has come to appreciate clearly the source of the energy of his electric motor. He sees that the consumption of zinc in the battery which drives the motor must be compared with the consumption of coal in the furnace that heats the steam-engine. With this realisation comes the disappointment of his hopes that the cost of working the motor might be reduced *ad infinitum*. In a lecture at the Victoria Gallery, Manchester, 16th February 1841, he says: "With my apparatus every pound of zinc consumed in a Grove's battery produced a mechanical force (friction included) equal

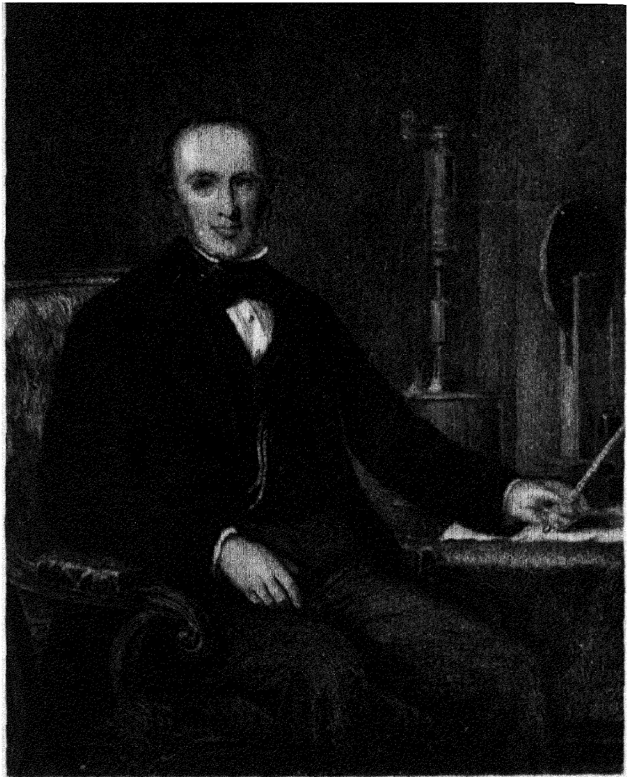
to raise 331,400 pounds to the height of 1 foot, when the revolving magnets were moving at the velocity of 8 feet per second. Now the duty of the best Cornish steam-engine is about 1,500,000 pounds raised to the height of 1 foot by the combustion of 1 pound of coal, which is nearly five times the extreme duty which I was able to obtain from my electromagnetic engine by the consumption of 1 pound of zinc. This comparison is so very unfavourable that I confess I almost despair of the success of electromagnetic attractions as an economical source of power; for although my machine is by no means perfect, I do not see how the arrangement of its parts could be improved so far as to make the duty per pound of zinc superior to the duty of the best steam-engine's per pound of coal. And even if this were attained, the expense of the zinc and exciting fluids of the battery is so great, when compared with the price of coal, as to prevent the ordinary electromagnetic engine from being useful for any but very peculiar purposes."

Transformation of Energy

This failure, however, is only the gateway leading to a success greater than any he had ever dreamed of. The practical problem he had been dealing with had suggested to his mind many philosophical problems. He had also realised how incomplete was the knowledge of the phenomena he had been dealing with. These two interests—the one philosophical, the other practical—combined to lead him to his great discovery.

Towards the end of 1840, at the age of twenty-two, he presented to the Royal Society a paper "On the Production of Heat by Voltaic Electricity." This paper had the distinction of being refused in its complete form, but was read in abstract and published in the *Proceedings*,

PLATE V



2nd That the quantity of heat capable of increasing the temperature of a lb of water (weighed in vacuo, and taken at between 55 and 60°) by one degree Fahr., requires for its evolution the expenditure of a mechanical force represented by the pressure of 772 lbs through the space of one foot.

J. P. Joule

FROM THE PAINTING BY G. PATTEN, IN THE POSSESSION OF THE MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY

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PLATE VI

PORTRAIT OF JOULE, IN THE POSSESSION OF THE ROYAL SOCIETY, FROM THE ENGRAVING BY JEENS.

compressed into twenty lines of printed matter. This reception of his work shows clearly how far in advance of the knowledge of his time it was. He links up the chemical effect in the battery, the mechanical effect in the motor, the electrical effect in the circuit, and shows that they are all numerically related. When 1 pound of zinc is dissolved in acid a certain amount of heat is evolved. If the zinc be made one element of a battery, Joule shows that less heat is evolved. If the current from the battery passes through a wire, it heats the wire. If this heat be measured, it will be found to be exactly the difference between the heat evolved when zinc is dissolved in the ordinary way and when it is dissolved in a battery. Further, if the current be used to drive a motor, some of the heat is missing again, and the amount which disappears from the account is in strict proportion to the work the motor does. To us, the bearing of these discoveries is abundantly evident. Zinc and dilute acid represent chemical energy. When the zinc is dissolved this energy appears as heat. When solution takes place in a battery the terminals of which are joined by a wire, an electric current flows. This current represents energy, and this energy has been derived from the solution of the zinc. Some of the chemical energy has therefore been converted into electrical energy, and the remainder appears as heat. If the electrical energy is allowed to dissipate itself in the wire, it is transformed into heat. All the chemical energy has once more been transformed into heat, but some appears in the battery and some in the wire. If now an electric motor is inserted in the circuit, some of the electrical energy is transformed into work, and so less heat appears in the circuit. All this Joule had begun to see, and all of it, and much more, his later work was destined to establish. But there were many difficulties to be met, and much

prejudice to be overcome before the new ideas came into their own.

JOULE'S EQUIVALENT

In 1843 he saw clearly for the first time the problem which he was so successful in solving and which absorbed his interest and his powers almost throughout his life. This problem was the relation between work and heat. We first find it definitely stated as the subject of investigation in a paper entitled "On the Calorific Effects of Magneto-Electricity and on the Mechanical Value of Heat," which was read at the meeting of the British Association at Cork in August 1843. In this paper he first deduces a value for what is now known as Joule's Equivalent—the amount of work which must be completely transformed into heat in order to give 1 unit. His unit of work was the foot-pound. His unit of heat was the amount of heat required to raise 1 pound of water 1° Fahr. in temperature. By comparing the work done in driving his magneto-electric engine with the heat generated in the wires by the resulting electric current, he found this equivalent to be 838—*i.e.* if 838 foot-pounds of work are completely converted into heat, the heat produced will raise the temperature of 1 pound of water by 1° Fahr. The accurate determination of this equivalent was a problem which filled all his thoughts. He approached it from every point of view that his fertile brain could suggest. He determined it with every refinement of accuracy which his remarkable experimental skill could contrive.

In 1843 also he measured the work required to drive water through fine tubes, and measured the heat produced. He found 770 as the value of the equivalent. In 1845 he measured the work done and the heat generated when air was compressed (Mayer's experiment);

also the heat lost and the work done when air was allowed to expand (Séguin's experiment). These experiments gave him 798. In the same year he measured the work required to revolve a paddle-wheel in water and the heat produced by the agitation of the water, and found 890. In 1847 he repeated this experiment with much more care and refinement, using both water and sperm oil, and obtained 781·5 in the former case and 782·1 in the latter. "The mean of the two results is 781·8, which is the equivalent I shall adopt until further and still more accurate experiments shall have been made." This is the concluding sentence of his paper "On the Mechanical Equivalent of Heat, as determined by the Heat evolved by the Friction of Fluids," read before the British Association at Oxford in June 1847.

Reception of Joule's Work

All this time Joule had been steadily maintaining, strengthening, and indeed establishing the unpopular view of the nature of heat. The attitude which the scientific man of his day adopted towards him at this time is indicated in a note dated 1885, which appears in volume ii. of his *Collected Papers* :

"It was in the year 1843 that I read a paper 'On the Calorific Effects of Magneto-Electricity and the Mechanical Value of Heat' to the Chemical Section of the British Association assembled at Cork. With the exception of some eminent men, among whom I recollect with pride Dr. Apjohn, the President of the Section, the Earl of Rosse, Mr. Eaton Hodgkinson, and others, the subject did not excite much general attention ; so that when I brought it forward again at the meeting in 1847, the chairman suggested that, as the business of the Section pressed, I should not read my paper but

confine myself to a short verbal description of my experiments. This I endeavoured to do, and, discussion not being invited, the communication would have passed without comment if a young man had not risen in the Section, and by his intelligent observations created a lively interest in the new theory. The young man was William Thomson, who had two years previously passed the University of Cambridge with the highest honour, and is now probably the foremost scientific authority of the age. My work with Thomson was chiefly experimental, performed in Manchester and the neighbourhood. We pursued the discussion of the thermal effects of fluids in motion until the experiments were interrupted by the action of the owners of the adjacent property, who, on the strength of an obsolete clause in the deeds of conveyance, threatened legal proceedings, the cost of which I did not feel disposed to incur."

The best possible testimony to the fundamental nature of the new views established by Joule is the slowness with which they won the assent of the greatest and most unprejudiced minds. The caloric theory was deeply rooted, and the adherence to it of Carnot was an important element in the situation. Carnot's work had already justified itself in the most convincing way in which a scientific theory can justify itself—by prediction. James Thomson, Lord Kelvin's brother, had used Carnot's conception of a reversible cycle of operations to show that the melting-point of ice must be lowered by pressure. Kelvin verified this prediction not only qualitatively but quantitatively. It is now a familiar phenomenon. If ice or snow which is near the freezing-point is subjected to pressure, its melting-point is lowered and it melts. When the pressure is released, the melting-point rises and it freezes again. Upon this obscure phenomenon depends the possibility of making good snowballs. The "binding"

is due to melting and refreezing when the snow is compressed in the hands. Hence also arises the impossibility of making good snowballs out of very cold snow. We cannot exert sufficient pressure to lower the melting-point to the actual temperature of the snow. The phenomenon also plays a part in skating—there is usually a film of water under the blade of the skate—and in the motion of glaciers.

But there were some difficulties in Carnot's view, and one of them was responsible for the coining in its modern sense of the word "energy." Carnot himself had seen the difficulty. If heat, merely by passing from a high temperature to a low temperature, can do work, and if heat is in fact constantly being given by colder bodies to hotter bodies in the ordinary operations of nature without work being done, what happens to its power of doing work? If, in winter, heat is passing all the time from the fire in our room to the cooler walls, furniture, etc., what becomes of the work it might have done? As Kelvin graphically puts it: "Nothing can be lost in the operations of Nature—no *energy* can be destroyed." What, then, is produced in place of the mechanical effect which is lost?

The meeting of Kelvin and Joule at the British Association in 1847 was an incident fraught with great consequences to the progress of physics. It was Kelvin who, in spite of a strong conviction of the truth of Carnot's theory, secured a fair hearing for Joule. In the end it was found that both were right. Work is done when heat passes from the boiler of a steam-engine to the cooler condenser, but *also* some of the heat disappears and the amount that disappears is exactly equivalent to the work done. Heat *is* converted into work only by being destroyed *as heat*, but *also* some of the heat must be passed from the boiler to the condenser. It can never be completely converted into work.

Kelvin and Joule

The meeting of Kelvin and Joule and their subsequent relationship is thus dealt with by Lord Kelvin himself (*Life of Kelvin*, vol. i. p. 264): "I can never forget the British Association at Oxford in 1847, when in one of the sections I heard a paper read by a very unassuming young man, who betrayed no consciousness in his manner that he had a great idea to unfold. I was tremendously struck with the paper. I at first thought it could not be true, because it was different from Carnot's theory, and immediately after the reading of the paper I had a few words with the author, James Joule, which was the beginning of our forty years' acquaintance and friendship. On the evening of the same day, that very valuable institution of the British Association, its conversazione, gave us opportunity for a good hour's talk and discussion over all that either of us knew of thermodynamics. I gained ideas which had never entered my mind before, and I thought I too suggested something worthy of Joule's consideration when I told him of Carnot's theory. Then and there in the Radcliffe Library, Oxford, we parted, both of us, I am sure, feeling that we had much more to say to one another and much matter for reflection in what we had talked over that evening. But a fortnight later, when walking down the valley of Chamounix, I saw in the distance a young man walking up the road towards me, and carrying in his hand something which looked like a stick, but which he was using neither as an alpenstock nor as a walking-stick. It was Joule with a long thermometer in his hand, which he would not trust by itself in the char-à-banc, coming slowly up the hill behind him, lest it should get broken. But there, comfortably and safely seated in the char-à-banc, was his bride—the sympathetic companion and sharer in his work

of after-years. He had not told me in Section A, or in the Radcliffe Library, that he was going to be married in three days, but now in the valley of Chamounix he introduced me to his young wife. We appointed to meet again a fortnight later at Martigny to make experiments on the heat of a waterfall (Sallanches) with that thermometer; and afterwards we met again and again, and from that time, indeed, remained close friends till the end of Joule's life. I had the great pleasure and satisfaction for many years, beginning just forty years ago, of making experiments along with Joule, which led to some important results in respect to the theory of thermodynamics. This is one of the most valuable recollections of my life, and is indeed as valuable a recollection as I can conceive in the possession of any man interested in science."

In a letter to Mr. J. T. Bottomley in 1882 (*Nature*, vol. xxvi. p. 618), Kelvin writes: "Joule's paper at the Oxford meeting made a great sensation. Faraday was there and was much struck with it, but did not fully enter into the new views. It was many years after that before any of the scientific chiefs began to give their adhesion. It was not long after when Stokes told me he was inclined to be a Joulite. Miller or Graham, or both, were for many years quite incredulous as to Joule's results, because they all depended on fractions of a degree of temperature—sometimes very small fractions. His boldness in making such large conclusions from such very small observational effects, is almost as noteworthy and admirable as his skill in extorting accuracy from them. I remember distinctly at the Royal Society, I think it was either Graham or Miller saying simply he did not believe Joule because he had nothing but hundredths of a degree to prove his case by."

The incident at Chamounix reveals Joule as quite small incidents frequently reveal us all. He had reasoned

that as, in coming over a fall, water lost potential energy and gained kinetic energy, and as, at the bottom of the fall this kinetic energy disappears, *therefore* its equivalent in heat must appear instead. Thus if 1 pound of water falls a height of 772 feet it loses 772 foot-pounds of energy. But 772 foot-pounds of energy (assuming this to be the correct value of Joule's equivalent) is the quantity of energy required to raise 1 pound of water 1° Fahr. Thus for a fall of 772 feet the water below the fall ought to be about 1° Fahr. warmer than the water above the fall. Here was another way of tackling his great problem, and neither the excitement of his wedding nor the attractions of his wedding trip could drive it from his mind.

Joule's Accuracy

The criticism of " either Graham or Miller," although a little exaggerated, is not altogether inexcusable. Knowing something of the inaccuracies of measurement in the days before Joule, we cannot but sympathise with their caution. But Joule knew what he was doing when he " made such large conclusions from such very small observational effects." Only his passion for accuracy could have justified him in making them.

In 1849 Joule returned to the attack, and made a new determination of J by the churning of water. We shall consider this paper more in detail in the next chapter. For the present it is sufficient to note that the value obtained as a result of these experiments was 772. They were repeated with modifications by Rowland about thirty years later, and in reviewing the position at that time Rowland says (*Proc. Amer. Acad. for Arts and Sciences*, 11th June 1879) : " We find that the only experimenter who has made the determination with anything like the accuracy demanded by modern science,

and by a method capable of giving good results, is Joule, whose determination of thirty years ago, confirmed by some recent results to-day, stands almost, if not quite, alone among accurate results on the subject." At the time when the work was done, Joule was thirty-one years of age.

In 1867 Joule redetermined J by an electrical method, and obtained 783. The discrepancy between this value and the 772 just mentioned was difficult to account for, and, to Joule especially, most unsatisfactory. It was impossible to let the matter rest there. In 1870 the meeting of the British Association appointed a Committee consisting of Kelvin, Tait, Clerk Maxwell, Balfour Stewart, and Joule to investigate the reasons for the discrepancy, and to ascertain if possible whether it was due to errors in Joule's thermal experiments or to an error in the electrical unit of resistance upon which the higher value depended.

Joule, equipped with much previous experience and increased experimental skill, embarked on a fresh determination by the method of churning, and published the results in 1878. The value obtained was 772·55—differing by less than one part in one thousand from the value obtained in 1849. Meantime Rowland had shown that the error lay in the unit of electrical resistance. Lord Rayleigh confirmed this in 1881 and 1882, and completely vindicated the accuracy of Joule's work by showing that the corrected value for his electrical method carried out in 1867 agreed almost exactly with the value of 772·55 obtained by the churning of water.

Tributes to Joule

Joule was elected a fellow of the Royal Society in 1850, and was awarded the Royal Medal in 1852 and the

Copley Medal in 1860. In presenting this latter medal, Sir Edward Sabine said: "The award of two medals for the same researches is an exceedingly rare proceeding in our Society—and rightly so. The Council have on this occasion desired to mark by it in the most emphatic manner their sense of the special and original character and high desert of Mr. Joule's discovery."

He was to have been President of the British Association in 1872, and again at the Manchester meeting in 1887, but ill-health prevented his attendance on both occasions. Indeed from 1872 until his death in 1889 the state of his health compelled him to live a quiet and uneventful life.

"I believe," he told his brother in 1887, "I have done two or three little things, but nothing to make a fuss about."

He is commemorated by portraits in the rooms of the Manchester Literary and Philosophical Society, by a statue in the Manchester Town Hall, and by a memorial tablet in Westminster Abbey, directly under the memorial to Sir George Stokes.

The following appreciation of Joule is taken from the Memoir prepared for the Manchester Literary and Philosophical Society by Professor Osborne Reynolds, and published in 1892:

"It was at a meeting of the Society in 1869 that the author first saw Dr. Joule, who was then in the chair. Although having of necessity become imbued with the transcendent importance of Joule's work in Physical Philosophy, and the appreciation in which this was held, and regarding him much in the same light as we regard Galileo or Newton, as being of another order, the impression on first sight contained no suggestion of disappointment. That Joule, who was then fifty-one years of age, was rather under the medium height; that he

was somewhat stout and rounded in figure; that his dress, though neat, was commonplace in the extreme; and that his attitude and movements were possessed of no natural grace, while his manner was somewhat nervous, and he possessed no great facility of speech, altogether conveyed an impression of the simplicity and utter absence of all affectation which had characterised his life; while his fine head and the reflective intelligence of his grave face accorded with the possession and long exercise of the highest philosophical powers. Another thing, too, calculated to impress a new member, was the obvious respect, amounting to veneration, that was plainly evinced by all the members present at the meeting, as was also the kindly and encouraging remarks which Joule, as president, made opportunity to address to the new member, either during the meeting or after it was over.

"Such were the first impressions, which were only strengthened by further and closer intercourse, extending over seventeen years. It soon became evident that it was not merely veneration arising from the fame of Joule that inspired the members of the Society, but that it was an attachment arising from the inherent lovability of his character—kindly, noble, and chivalrous in the extreme, and though modest and absolutely devoid of mere personal ambition; yet jealous for the interests of his friends and the Society in general, and, in particular, jealous in the interest of everything truly scientific. Anything that looked like ostentation or quackery excited Joule's indignation, particularly when exhibited by those possessing the popular ear. On the other hand, he always noticed with encouragement the efforts of those who were yet unknown, and resented any attempt at the disparagement of their work—as though his own early experience had left him with a fellow-feeling with those who were struggling to get their views taken up."

CHAPTER VII

JOULE'S PAPER

"On the Mechanical Equivalent of Heat."

By JAMES PRESCOTT JOULE, F.C.S., Sec. Lit. and Phil. Society, Manchester; Cor. Mem. R.A., Turin, etc. (Communicated by MICHAEL FARADAY, D.C.L., F.R.S., Foreign Associate of the Academy of Sciences, Paris, etc. etc. etc.).

[*Philosophical Transactions*, 1850, Part I. Read June 21, 1849.]

"Heat is a very brisk agitation of the insensible parts of the object, which produces in us that sensation from whence we denominate the object hot; so what in our sensation is heat, in the object is nothing but *motion*."—Locke.

"The force of a moving body is proportional to the square of its velocity or to the height to which it would rise against gravity."—Leibnitz.

The paper begins with these two quotations. For the understanding of the latter, and of the paper which follows, we must remember that what we now call "energy" was then known as "force."

References to Earlier Work

In the introductory paragraph Joule reviews the work which has led up to his research. Rumford is quoted in support of the general idea that heat is motion, but he is also interpreted so as to give support of a more direct kind. "One of the most important parts of Count Rumford's paper, though one to which little attention has hitherto been paid, is that in which he makes an estimate of the quantity of mechanical force required to produce a certain amount of heat. Referring to his third

experiment, he remarks that the *total quantity of ice-cold water which, with the heat generated by friction and accumulated in 2 hours 30 minutes, might have been heated* 180°, *or made to boil*=26·58 *pounds*. In the next page he states that *the machinery used in the experiment could easily be carried round by the force of one horse* (*though, to render the work lighter, two horses were actually employed in doing it*). Now the power of a horse is estimated by Watt at 33,000 foot-pounds per minute, and therefore if continued for two hours and a half will amount to 4,950,000 foot-pounds, which, according to Count Rumford's experiment, will be equivalent to 26·58 pounds of water raised 180°. Hence the heat required to raise 1 pound of water 1° will be equivalent to a force represented by 1034 foot-pounds. This result is not very widely different from that which I have deduced from my own experiments related in this paper, namely, 772 foot-pounds; and it must be observed that the excess of Count Rumford's equivalent is just such as might have been anticipated from the circumstance, which he himself mentions, that *no estimate was made of the heat accumulated in the wooden box, nor of that dispersed during the experiment*." Sir Humphry Davy, Mayer, and Séguin are all cited, and his own earlier work summarised.

Description of Apparatus

Next follows the description of his apparatus—clear, and almost laborious in its minute detail. The three thermometers come first, and he designates them A, B, and C, recording their makers and giving the maker also of the standard thermometer with which, as a preliminary, they are carefully compared. They were evidently not direct-reading thermometers such as we use now, because "it was thus found that the number of divisions corre-

sponding to 1° Fahr. in the thermometers A, B, and C were 12·951, 9·829, and 11·647 respectively." These are evidently mean values, as they are given to the one-thousandth part of a division, which even Joule does not claim to estimate. Most observers flatter themselves that with a little practice they can estimate tenths of a division whatever the size of the division may be. Joule, however, says, "and since constant practice had enabled me to read off with the naked eye to $\frac{1}{20}$ of a division, it followed that $\frac{1}{200}$ of a degree Fahr. was an appreciable temperature." To understand the importance of the emphasis he lays on the accuracy with which his thermometer graduations have been determined, and the accuracy with which he can take his readings, we must bear in mind that he was estimating the heat produced in his apparatus by a rise of temperature, and that the whole rise which he obtained was only about half a degree. Thus an error of $\frac{1}{200}$ of a degree in his readings would be an error of one part in a hundred, or an error of 1 per cent. in his result.

Next comes the description of the apparatus itself, figured in the accompanying plate. "Plate II. Fig. 69 represents a vertical and Fig. 70 a horizontal plan of the apparatus employed for producing the friction of water, consisting of a brass paddle-wheel furnished with eight sets of revolving arms *a*, *a*, etc., working between four sets of stationary vanes *b*, *b*, etc., affixed to a framework also in sheet brass. The brass axle of the paddle-wheel worked freely but without shaking, on its bearings at *c*, *c*, and at *d* was divided into two parts by a piece of boxwood intervening, so as to prevent the conduction of heat in that direction.

"Plate II. Fig. 71 represents the copper vessel into which the revolving apparatus was firmly fitted; it had a copper lid, the flange of which, furnished with a very thin washer of leather saturated with white lead, could be

screwed perfectly watertight to the flange of the copper vessel. In the lid there were two necks *a*, *b*; the former for the axis to revolve in without touching, the latter for the insertion of the thermometer.

"Besides the above, I had a similar apparatus for experiments on the friction of mercury, which is represented by Plate II. Figs. 72, 73, and 74. It differed from the apparatus already described in its size, number of vanes (of which six were rotary and eight sets stationary), and material, which was wrought iron in the paddle-wheel, and cast iron in the vessel and lid.

"Being anxious to extend my experiments to the friction of solids, I also procured the apparatus represented by Plate II. Fig. 75, in which *aa* is the axis revolving along with the bevelled cast-iron wheel *b*, the rim of which was turned true. By means of the lever *c*, which had a ring in its centre for the axis to pass through, and two short arms *d*, the bevel-turned cast-iron wheel *e* could be pressed against the revolving wheel, the degree of force applied being regulated by hand by means of the wooden lever *f* attached to the perpendicular iron rod *g*. Fig. 76 represents the apparatus in its cast-iron vessel.

"Plate II. Fig. 77 is a perspective view of the machinery employed to get the frictional apparatus just described in motion. *aa* are wooden pulleys, 1 foot in diameter and 2 inches thick, having wooden rollers *bb*, *bb*, 2 inches in diameter, and steel axles *cc*, *cc*, one quarter of an inch in diameter. The pulleys were turned perfectly true and equal to one another. Their axles were supported by brass friction-wheels *dddd*, *dddd*, the steel axles of which worked in holes drilled into brass plates attached to a very strong wooden framework firmly fixed into the walls of the apartment. This was a spacious cellar, which had the advantage of possessing a uniformity of temperature far superior to that of any other laboratory I could have used.

"The leaden weights *e*, *e*, which in some of the ensuing experiments weighed about 29 pounds and in others about 10 pounds apiece, were suspended by string from the rollers *bb*, *bb*; and fine twine attached to the pulleys *aa* connected them with the central roller *f*, which, by means of a pin, could with facility be attached to, or removed from, the axis of the frictional apparatus."

Joule was fully alive to the fact that as far as possible his apparatus must be "insulated" for heat. He has already described the boxwood insertion in the axle to prevent conduction of heat in that direction. He now notes two other similar precautions.

"The wooden stool *g* upon which the frictional apparatus stood was perforated by a number of transverse slits, so cut out that only a very few points of wood came in contact with the metal, whilst the air had free access to almost every part of it. In this way the conduction of heat to the substance of the stool was avoided.

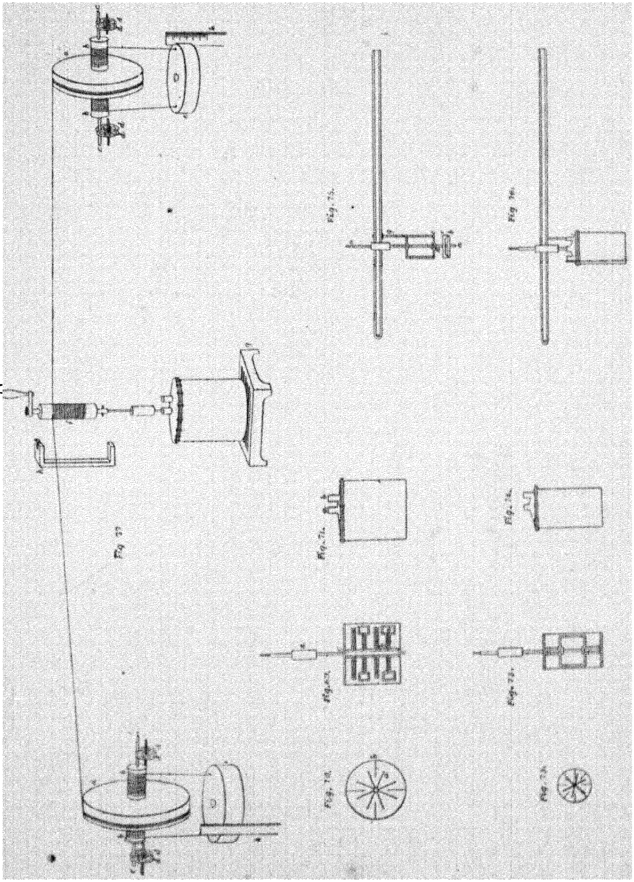
"A large wooden screen (not represented in the figure) completely obviated the effects of radiant heat from the person of the experimenter."

Method of Experiment

The design of the experiment is ideally simple. The friction is produced by the fall of weights. If we know the weights and the height through which they fall, we can at once calculate the number of units of work done. The friction heats the water and the containing vessel. If we measure the rise of temperature we can calculate the number of units of heat produced. Thus both work and heat are determined directly and their equivalence can be at once deduced.

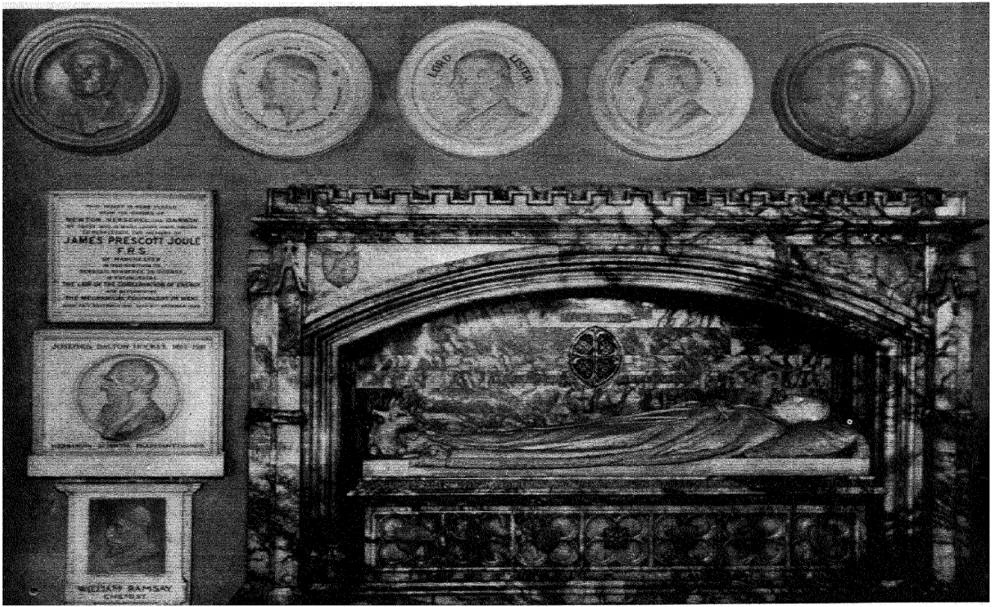
"The method of experimenting was simply as follows: The temperature of the frictional apparatus having been

PLATE VII



JOULE'S APPARATUS, FROM THE DIAGRAM IN HIS "COLLECTED PAPERS."

PLATE VIII



TABLET TO JOULE IN WESTMINSTER ABBEY, NEAR THE BURIAL-PLACES OF NEWTON AND KELVIN.

Below are tablets to Sir JOSEPH HOOKER (botanist) and Sir WILLIAM RAMSAY (chemist). Above are tablets to Sir GEORGE STOKES (physicist), JOHN COUCH ADAMS (astronomer); Lord LISTER (surgeon); ALFRED RUSSEL WALLACE (biologist); CHARLES DARWIN (biologist).

ascertained and the weights wound up with the assistance of the stand *h*, the roller was refixed to the axis. The precise height of the weights above the ground having then been determined by means of the graduated slips of wood *k*, *k*, the roller was set at liberty and allowed to revolve until the weights reached the flagged floor of the laboratory, after accomplishing a fall of about 63 inches. The roller was then removed to the stand, the weights wound up again, and the friction renewed. After this had been repeated twenty times, the experiment was concluded with another observation of the temperature of the apparatus. The mean temperature of the laboratory was determined by observations made at the commencement, middle, and termination of each experiment."

So far, conduction of heat along the axle and to the stand has been almost completely prevented, the disturbance due to the observer's person has been eliminated, but what of the heat radiated from or to the surroundings? If the vessel is warmer than its surroundings, it will radiate heat to them; if cooler, it will receive heat from them. This radiation effect may be minimised by making the temperature of the vessel differ as little as possible from that of the surrounding air, but the difficulty cannot be completely got rid of in this way. Joule recognises the importance of this factor and proposes to deal with it as follows:

"Previously to, or immediately after, each of the experiments I made trial of the effect of radiation and conduction of heat to or from the atmosphere in depressing or raising the temperature of the frictional apparatus. In these trials the position of the apparatus, the quantity of water contained by it, the time occupied, the method of observing the thermometers, the position of the experimenter, in short everything, with the exception of the

apparatus being at rest, was the same as in the experiments in which the effect of friction was observed."

Correction of Data

The experiments divide themselves into five series, of which we shall consider the first in some detail and content ourselves with a summary of the results of the others.

"Series 1.—Friction of water. Weight of the leaden weights along with as much of the string in connexion with them as served to increase the pressure, 203,066 grains and 203,086 grains." (Note that even the weight of the string is not neglected.)

"Velocity of the weights in descending, 2·42 inches per second. Time occupied by each experiment, 35 minutes. Thermometer employed for ascertaining the temperature of the water, A. Thermometer for registering the temperature of the air, B."

Here follows a table of the results of forty experiments set out as in the one quoted below, and ending with the mean results for the whole series of forty experiments.

No. of Experiment and Cause of Change of Temperature.	Total Fall of Weights in Inches.	Mean Temperature of Air.	Difference between Mean of Columns 5 and 6 and Column 3.	Temperature of Apparatus.		Gain or Loss of Heat during Experiment.
				Commencement of Experiment.	Termination of Experiment.	
1 Friction . .	1256·96	57°·698	2°·252 –	55°·118	55°·774	0°·656 gain
1 Radiation .	0	57°·868	2°·040 –	55°·774	55°·882	0°·108 gain
Mean Friction .	1260·248	...	0·305075	...	...	0·575250 gain.
Mean Radiation .	0	...	0·322950	...	...	0·012975 gain.

From this table we gather that the series started with a friction experiment in which the total fall of the weights was 1256·96 inches. The initial temperature of the water in the vessel was 55°·118 and the final temperature 55°·774, showing a rise of temperature of 0°·656. The apparatus is now left to itself for a time, equal to that occupied by the friction experiment. It is below the temperature of the air and so is receiving heat, as of course it was throughout the friction experiment. Its temperature at the end of this radiation experiment is found to be 55°·882, showing that it has received heat equivalent to 0°·108 rise of temperature by radiation alone. This is almost one-sixth of the rise of temperature due to the friction.

Calculation of Results

Now let us see how Joule deals with his data. Clearly we require to know (1) the amount of heat generated, (2) the amount of work done. In order to calculate the amount of heat generated we must know (*a*) the rise of temperature, (*b*) the number of pounds of water to which the calorimeter and contents are equivalent.

1 (*a*). The mean rise of temperature for the forty experiments is 0°·575250 Fahr. But the mean temperature of the apparatus in the experiments has been below the temperature of the atmosphere, so that some of this increase of temperature is due to heat which has passed into the apparatus from the warmer atmosphere. We see that in the experiments in which the paddle was not working (the radiation experiments) an average difference between the mean temperature of the water and the temperature of the air amounting to 0°·322950 has resulted in a rise of temperature of 0°·012975. Now the heat communicated from the atmosphere in a given time is known to be proportional to the mean difference of

temperature between the atmosphere and the vessel. Correcting for this, Joule finds that of the observed rise $0^{\circ}\cdot563209$ is due to the work done in the water, and $0^{\circ}\cdot012041$ is due to radiation.

1 (*b*). The calorimeter was of copper, 1 grain of which in rising through any given range of temperature absorbs as much heat as 0·09515 grains of water. Its mass was 25,541 grains. Its "water equivalent" is therefore $25{,}541 \times 0\cdot09515 = 2430\cdot2$ grains. The brass of which the paddle-wheel is constructed is next analysed to find the proportion of copper and zinc and the water equivalent calculated thus :

cap. 14,968 grains copper	× 0·09515	=cap. 1424·2 grains water.
cap. 3,933 grains zinc	× 0·09555	=cap. 375·8 grains water.
Total capacity brass wheel		=cap. 1800 grains water.

Last comes a brass stopper which is calculated to have the capacity of 10·3 grains of water. Adding all these, we have

Contained water	93,229·7	grains.
Calorimeter as water	2,430·2	,,
Paddle-wheel as water	1,800·0	,,
Stopper as water	10·3	,,
	97,470·2	,,

2. The work done in generating this heat will be the product of the weights used to drive the paddle-wheel and the total distance through which they were allowed to fall. The weights amounted to 406,152 grains. This weight, however, is not all effective in driving the paddle-wheel. Some of it is required to bend the string round the pulley-wheels which carry it and to rotate the wheels. A separate experiment is performed to determine the amount to be deducted on this account, and it is found to be 2837 grains. The effective weight is therefore 403,315 grains. But all the work developed by the fall

of the weights through the observed height is not communicated to the apparatus. When the weights reach the floor they are moving with a velocity estimated by Joule at 2·42 inches per second. The kinetic energy which they possess in virtue of this velocity they communicate to the floor at the moment of impact. Now they would acquire a velocity of 2·42 inches per second in falling freely through 0·0076 inch. This distance must therefore be subtracted from the observed height through which they fall in calculating the work communicated to the paddle-wheel. The observed height is 63·0124 inches, and as the weights are wound up and released 20 times in each experiment the total fall

$$=20(63{\cdot}0124 - 0{\cdot}0076) \text{ inches} = 1260{\cdot}096 \text{ inches.}$$

Multiplying the corrected weight given above by this corrected height, we get the work done, and reducing the result to feet and pounds as units, we obtain 6050·186 foot-pounds. But Joule recognises the necessity for still another correction. He notices that there is some work done on the paddle-wheel after the weights have reached the ground. While the weights are falling, the string which suspends them is stretched. When the weights reach the ground the string is relieved of the stretching force and contracts, doing work on the paddle. This correction is estimated by Joule at 16·928 foot-pounds, and adding this to the corrected value previously obtained we have 6067·114 foot-pounds.

From 1 (*a*) and 1 (*b*) we see that the heat evolved raises 97470·2 grains of water through $0^\circ{\cdot}563209$ Fahr.; it would therefore raise 7·842299 pounds of water 1° Fahr. To generate this heat 6067·114 foot-pounds of work are done.

"Hence $\dfrac{6067{\cdot}114}{7{\cdot}842299} = 773{\cdot}64$ foot-pounds will be the

force (*i.e.* work) which, according to the above experiments on the friction of water, is equivalent to 1° Fahr. in a pound of water."

Summary of Experiments

Having discussed in some detail this—the first—series of experiments reported in the paper, it will be unnecessary to do more than summarise the remaining series very briefly.

Series 2.—Twenty experiments on the heat generated by friction of mercury, leading to 773·62 as the mechanical equivalent.

Series 3.—Thirty experiments on the heat generated by friction of mercury, giving 776·303.

Series 4.—Ten experiments on heat generated by the friction of cast-iron bevelled wheels, giving 776·997.

Series 5.—Ten experiments using the same cast-iron wheels, but driving them with lighter weights. The result this time is 774·88.

Conclusion

These results show a quite remarkable agreement in view of the difficulties of the experiments and the diversities of the methods. They are summarised in a table, and corrected for air buoyancy in view of the fact that a mass of 1 pound weighs less in air than in vacuo.

No. of Series.	Material Employed.	Equivalent in Air.	Equivalent in Vacuo.	Mean.
1	Water	773·640	772·692	772·692
2	Mercury	773·762	772·814	774·083
3	Mercury	776·303	775·352	
4	Cast iron	776·997	776·045	774·987
5	Cast iron	774·880	773·930	

The paper concludes thus: "It is highly probable that the equivalent from cast iron was somewhat increased by the abrasion of particles of the metal during friction, which could not occur without the absorption of a certain quantity of force in overcoming the attraction of cohesion. But since the quantity abraded was not considerable enough to be weighed after the experiments were completed, the error from this source cannot be of much moment. I consider that 772·692, the equivalent derived from the friction of water, is the most correct, both on account of the number of experiments tried and the great capacity of the apparatus for heat. And since, even in the friction of fluids, it was impossible entirely to avoid vibration and the production of a slight sound, it is probable that the above number is slightly in excess. I will therefore conclude by considering it as demonstrated by the experiments contained in this paper—(1) that the quantity of heat produced by the friction of bodies, whether solid or liquid, is always proportional to the quantity of force (*i.e.* work) expended; and (2) that the quantity of heat capable of increasing the temperature of a pound of water (weighed in vacuo, and taken at between 55° and 60°) by 1° Fahr., requires for its evolution the expenditure of a mechanical force represented by the fall of 772 pounds through the space of 1 foot."

CHAPTER VIII

DEVELOPMENT OF LAW OF CONSERVATION OF ENERGY

The Law

The paper which we have just discussed in detail was sufficient of itself to establish what is now known as the First Law of Thermodynamics—namely, that there is a definite numerical equivalence between heat and work, and that they are mutually convertible. Joule's other work not only confirmed this conclusion, but extended the range of mutually convertible forms of energy to include the energy of chemical combination and the energy of the electric current. Indeed, it laid the foundation of one of the widest generalisations of modern physics—the Law of Conservation of Energy. This law states that energy can neither be created nor destroyed, although it may be transformed into any of the numerous forms of which energy is susceptible.

Forms of Energy

Let us look first at some of these forms of energy, and then at the series of transformations by which they may be converted the one into another.

(*a*) Mechanical Energy.—With this form of energy we have already made ourselves familiar. We find it as the head of water which works a mill-wheel or a turbine. We find it in the weight which maintains the clockwork of the old grandfather clock or sometimes the revolving

lantern of the lighthouse. We find it in the coiled spring which drives the gramophone and in the waterfall, which, by a series of transformations, is made to distribute electric-power over a wide tract of country.

(*b*) HEAT.—This is the immediate source of energy in all steam-engines, which are merely mechanical contrivances for converting heat into work.

(*c*) CHEMICAL ENERGY.—When certain chemical actions take place, a large amount of energy may be liberated. When fuel is burned in a furnace or petrol is exploded in a cylinder, the energy is liberated as heat. When zinc is dissolved in a battery, the energy is liberated partly in the form of an electric current. The energy by the use of which animals maintain their activity is stored as chemical energy in food.

(*d*) WAVE MOTION.—All waves are carriers of energy. In the case of water waves this energy is usually made evident by its destructive effect in breaking up sea walls and in the erosion of cliffs. Sound waves also carry energy, and Joule notes that when the weights of his apparatus strike the floor, the resulting noise carries away a certain small fraction of the energy for which he finds it impossible to give any numerical estimate. Some of the large sirens round our coasts which are used as fog signals absorb several hundred horse-power while sounding. Most important of all, however, is the energy of the waves which come to us through space from the sun. When they fall on a surface which absorbs them, their energy is transformed into heat. We shall see later what a large part these waves play in the economy of the world.

TRANSFORMATION OF ENERGY

Let us consider now some of the typical and familiar series of transformations which energy may undergo.

Suppose we take the production of artificial light by electricity. We may start with chemical energy stored up in coal. When this coal is burned, the chemical energy disappears and heat energy takes its place. This heat passes from the furnace to the boiler of a steam-engine where it is used to evaporate water into steam. Of the heat thus used some is transferred to the condenser and disappears from our account. The difference between the heat taken into the boiler and the heat transferred to the condenser is, with certain losses to be considered later, transformed into mechanical energy. This mechanical energy is used to drive a dynamo which transforms it into electrical energy. This energy is transformed into ether waves in passing through the electric lamps. Some of these ether waves are invisible and are reflected from surface to surface in the room, a certain proportion of them being absorbed at each reflexion and transformed into heat. Others—and these are the waves we really wish to produce—affect the retina of the eye and so are visible. They too are reflected from surface to surface in the room, and it is by these reflected waves that the objects in the room are visible. At each surface a certain proportion of the energy they carry is absorbed as heat—the proportion being large for black surfaces and small for bright surfaces—and by these repeated reflexions the whole of the energy is finally transformed into heat. The several forms assumed by the energy are thus :

Chemical → Heat → Mechanical → Electrical → Light → Heat.

Let us consider next the series of transformations associated with the gramophone. Here we may start with the "human" energy which enables us to wind up the instrument. In this process the energy is transformed into potential mechanical energy of the coiled spring. This in turn, as the spring unwinds, is transformed into

the kinetic energy of the rotating disc. A fraction of this energy, still in the kinetic form, is transmitted to the needle and attached diaphragm, and a fraction of this again is converted into the energy of sound waves. Like the light waves these are reflected from surface to surface, and finally become heat. Here, then, we have another series :

"Human" energy→potential mechanical energy→kinetic energy→sound waves→heat.

Degradation of Energy

One fact about these two series is full of significance. The last term in each case is heat. This is not accidental. It is true of all series of energy transformations—they all end with heat as their last term. More than this, at each successive transformation the change is never complete, some of the energy becomes heat. Also any form of energy can be completely transformed into heat, but heat can only be partially transformed back into any other form of energy. Thus in the case investigated by Joule, work was completely transformed into heat, but the reverse process is never complete. Heat is only *available* for transformation into another form of energy when we have two bodies at different temperatures, and in addition to the heat which disappears and is transformed into work some heat passes from the high temperature (*e.g.* the boiler) to the low temperature (*e.g.* the condenser) unchanged. If we follow the fortunes of 1 gram of coal in the first series of transformations above, we shall see how this works out. The chemical energy in the coal can be found by burning completely a known weight of coal and measuring the heat produced. The energy in 1 gram of coal is thus found to be equivalent to a quantity of heat which would raise 8000 grams

of water through 1° Centigrade. The heat which would raise 1 gram of water 1° C. being defined as the unit of heat or calorie, we have 8000 calories available. Unfortunately combustion in the furnace is never complete, and in the unburnt fuel of the cinders we lose 240 calories. In the hot gases which escape up the flue we lose 1200 calories. In the heat radiated from the boiler and furnace we lose 320 calories. Actually, then, we communicate to the steam 6240 calories, having lost 1760 calories, or about 22 per cent. Three per cent. has remained in the fuel, 19 per cent. has been lost as heat. But the next stage is still more disastrous. By radiation from the cylinder of the engine we lose 400 calories, and carried away to the condenser in exhaust steam we lose 4800 calories ! This only leaves us 1040 calories out of our original 8000 calories. Our steam-engine only converts into mechanical energy 13 per cent. of the energy originally present in the coal. The working parts of the steam-engine are of course not quite frictionless, and the friction of the parts develops heat which is radiated away and lost. In this way we lose another 160 calories, and actually deliver to the dynamo 880 calories. The dynamo parts also work with some friction, and so the electrical energy delivered is equivalent to 800 calories. Seven thousand two hundred calories have escaped as heat, and only 800, or 10 per cent., of the original energy is available. When this electrical energy is finally transformed in a metallic filament electric lamp, about 12 calories are radiated as visible ether waves, and 788 calories as invisible ether waves. Thus the light energy we obtain in the end represents 0·15 per cent. of the original energy of the fuel. We have obtained 12 calories of " light " energy from 8000 calories of chemical energy. These figures are, of course, only approximate, but they give a good idea of the losses of transformation.

If we were to trace any other series of energy transformations we should find differences of detail, but the main trend would be the same. There is a tax—payable only in the form of heat—exacted at each step. Thus all kinds of energy tend to assume the form of heat.

Further, heat always tends to distribute itself at a uniform temperature. If two bodies at different temperatures are placed in contact, heat passes from the hotter to the cooler until the two bodies have reached a common temperature. Thus the ultimate fate of the world, so far as we can foresee it at present, is that all its energy will assume the form of heat and all the heat will be distributed at a uniform temperature. Now heat can only be transformed back into other forms of energy when there is a difference of temperature, so that all transformations of energy will then have ceased, including those essential to life itself, and all activity will have come to an end. This tendency—known as the "dissipation" or "degradation" of energy, was first pointed out by Lord Kelvin, and is an important qualification of the Law of Conservation of Energy. Hitherto unsuspected sources of energy may be discovered, but unless such discoveries are accompanied by the invention of some new process of transformation, not subject to the law of degradation, they can do no more than postpone the inevitable end.

CHAPTER IX

NATURAL SOURCES OF ENERGY

In view of these facts, the question of natural sources of energy becomes one of first-rate importance, and it behoves us to consider the sources at our disposal and the economy of the methods by which we attempt to utilise them. Among the sources so far explored we find the following:

1. Food

This is the source of energy of living things. The energy expended by the human body in its daily work, in its games, in its locomotion, is stored up in the food as chemical energy, and the body transforms this energy partly into the heat required to maintain its temperature, and partly into the mechanical energy necessary for its activity. The body in this respect resembles very closely a heat engine. The food is the fuel, and its energy value may be stated in the same units as that of fuel. During the war, when the nation's food supply gave a good deal of anxiety, attention was directed to the energy value of the various food-stuffs in common use, and this value was always stated in calories—*i.e.* as the number of units of heat obtained from the complete combustion of the food. The calorie commonly used as the unit in this connexion is the so-called "great calorie"—the heat required to raise 1000 grams of water through 1° C. It is thus equal to 1000 of the smaller calories. Using this "great calorie" as our unit, we find that a man

doing manual work must be supplied with about 3000 calories per day. A man pursuing a sedentary occupation will find about 2500 calories sufficient, while a sick patient in bed can be maintained on about 1800. The food-stuffs we ordinarily consume contain three main classes of chemical compound available for transformation in the body. These are (*a*) proteins, complicated substances containing nitrogen, taken mainly in the form of meat and fish; (*b*) carbohydrates, comprising sugars and starch, taken mainly as sugar, potatoes, and bread; (*c*) fat. The energy value of these types of food are, roughly: protein, 4·1 calories per gram; carbohydrates, 4·1 calories per gram; fat, 9·3 calories per gram.

The energy value of some common articles of diet is given in the following table, taken from *Food*, by Robert Hutchinson. The figures represent the number of calories given by 1 pound of the food:

Food.	Calories as Protein.	Calories as Fat.	Calories as Carbo-hydrate.	Total Calories per Pound.
Butter	18	3559	None	3577
Peas	418·8	71·7	982·5	1473
Cheese	553	750	None	1303
Bread	130	21·5	976·5	1128
Eggs	232	507	None	739
Beef	391	232	None	623
Potatoes	18·5	9·3	341·2	369
Milk	67	168	87	322
Fish (cod)	299	16	None	315
Apples	9	None	229	238

Some of these figures are, of course, quite approximate.

The daily requirements of the human body vary as we have seen with conditions of work, and they also vary with the season of the year, age, etc., but we can obtain a rough estimate as follows: The work done in raising

100,000 kilograms through a height of 1 metre represents a moderate amount of muscular activity for a day, and requires about 210 calories. Of the energy supplied to the body as food, however, only about one-fifth can be transformed into mechanical work, so that for the work suggested about 1000 calories is required. The maintenance of the body, when inactive, requires about 2000 calories, so that the total daily requirement is about 3000 calories. For reasons which have nothing to do with energy it is essential to have a mixed diet, and the necessary calories can be obtained by the following distribution :

120 grams protein (=¼ pound) . .	500	calories.
60 grams fat (=⅛ pound) . . .	500	,,
500 grams carbohydrates (=1 pound) .	2000	,,
Total . .	3000	,,

It is interesting to note the part played by plants in making this energy available for our use. The energy stored up in the chemical compounds produced by the plants is the main source of the energy of our food whether we feed directly on the plants or on animals which in their turn have fed on the plants. But what is the ultimate source of this energy ? It comes to the earth as light waves radiated from the sun. This is absorbed by the green chlorophyll of the plant, which, taking water from the soil and carbon dioxide from the air, builds up the chemical compounds in which the energy is stored. The green vegetation which abounds everywhere thus acts as a trap in which the energy reaching us from the sun can be stored and made available as food.

Fuels

First among the sources of energy available for industrial purposes we must place coal. The energy has, of

course, been stored by the vegetation of bygone ages, and the time required by Nature for the preparation of a coal seam is not to be measured in hundreds nor even in thousands of years. The coal-fields of the world represent accumulated capital which is being squandered by our spendthrift generation. It has been estimated that at the present rate of consumption the coal supply of the world would last for about 2500 years. The rate of consumption does not remain constant, however. It is steadily increasing, and if we take this increase into account we can foresee the exhaustion of the coal supply of the whole world in something like 350 years. Nor do we take any special pains to secure that our use of this valuable substance shall be as economical as possible. The coal burned in the open grates of our living-rooms gives about 12 per cent. of its heat usefully, while about 88 per cent. is wasted. In many of our cooking-ranges not more than 5 per cent. of the heat of the burning coal is really utilised. In many of our steam-engines we require about 5 pounds of coal per horse-power developed, while in the best only 1½ pounds to 2 pounds is necessary. We have only to contemplate the gradual paralysis of our whole national life during a coal strike to realise the importance of this commodity and the paramount necessity of using it as carefully and as economically as possible. Another fuel which has recently increased greatly in importance is oil. Like coal, the available supply is accumulated capital and is being rapidly exhausted. It may be used more or less directly as a substitute for coal, in which case it can generate about 50 per cent. more energy per pound. It is, however, much more economically used in the internal combustion engine, in which case 1 pound of oil will generate as much mechanical energy as 3½ pounds of coal consumed in a good steam plant.

SOLAR HEAT

This seems at first sight a particularly obvious and copious source of energy. The amount of solar heat received by the earth's surface between latitude 45° N. and latitude 45° S. is the equivalent of 8000 foot-pounds per minute per square foot. If this could be completely converted into mechanical work it would supply 1 H.P. for every 4 square feet of surface. An attempt has been made to focus the rays of the sun on to a boiler by the use of large parabolic mirrors. The plant, however, is costly to instal and costly to run, nor is the efficiency very high. Where coal is not found and transport charges make its price high, the solar heat plant may become a practical proposition, and already plants are in existence which develop 1 H.P. per 100 square feet of surface at a cost not greatly in excess of that obtained from a coal steam plant.

WATER-POWER

This is another source of energy with great possibilities which so far have not been at all fully utilised. The two main factors are obviously the amount of rainfall and the available height of fall, and the suitable combination of these is confined to a comparatively few localities. Where the conditions are exceptionally favourable the resulting power is cheap, and its price may not exceed about one-tenth of the cost of power developed from coal. The power may, of course, be transformed into electrical power and economically transmitted for use at a considerable distance. It is fairly certain that in the course of the next few decades every waterfall of reasonable size will be exploited and made to yield its quota of energy to the world's supply.

Tidal Energy

The most obvious method of using this source of energy is to imprison water at high tide and let it flow out through turbines at low tide. If attempts were made to utilise it in this simple way, difficulties would arise through the variation of water-head and of the period of the day during which energy would be available. Owing to the change daily in the time of high water there would be times when the whole of the ordinary working day would fall within a period during which the turbines were idle. These difficulties may be mitigated but they cannot be altogether overcome, and they affect adversely the economical use of tidal power. The enclosure of the necessary areas would only be possible where the coast-line was favourable and would in any case involve heavy initial outlay. Nevertheless, this is a source of energy to be reckoned with, and the possibility of constructing a large installation for the purpose in the estuary of the Severn is now under the consideration of a Government Committee.

Wind-Power

For small units and occasional working this is by far the most economical source of energy, and in Norfolk the fens are largely kept drained by the use of windmills which pump the water from the ditches surrounding and intersecting the fields into the rivers where the water level is several feet higher. But the wind is a fickle and uncertain source, not to be relied upon for a constant output, and the design of a windmill large enough to deliver a high output, slight enough to respond to light winds and strong enough to withstand storms, presents serious and obvious difficulties.

Atomic Energy

The discovery of radium has made us aware of the enormous stores of energy contained in its atoms and probably in the atoms of other substances. Calculation shows that the energy released by the spontaneous disintegration of 1 gram of radium is of the order of $1{\cdot}6 \times 10^9$ calories. This is 2×10^5 or two hundred thousand times the energy available from the burning of the same weight of coal. Radium, is of course, a very rare element, but there is no reason to believe that it is unique in the high energy content of its atom. Thus it has been calculated also that the transformation of 1 gram of hydrogen into helium—and the transmutation of the elements is now an accomplished fact—would liberate $1{\cdot}65 \times 10^{11}$ calories, or about one hundred times the amount of energy liberated by the spontaneous disintegration of 1 gram of radium. At present we are not even in sight of any method by which these enormous stores of energy may be tapped, but it goes without saying that should they ever become available the whole aspect of civilisation would be changed.

Conclusion

Such are the results of a short survey of our natural sources of energy. On the one hand, we cannot but hope that as our present sources of energy are exhausted we may succeed in making new sources available. On the other hand, we may be forgiven if we feel some doubt whether mankind has reached that stage of moral and spiritual development at which the discovery of vast new sources of energy and power would be certain to be used for the enrichment and beautifying of human life.

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